THE MATHEMATICAL GAZETTE

EDITED BY
W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF
F. S. MACAULAY, M.A., D.Sc.
AND
Prof. E. T. WHITTAKER, M.A., F.R.S.

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NOTICE.

The following Reports have been issued by the Association:—(i) "Revised Report on the Teaching of Elementary Algebra and Numerical Trigonometry" (1911), price 3d. net; (ii) "Report on the Correlation of Mathematical and Science Teaching," by a Joint Committee of the Mathematical Association and the Association of Public School Science Masters, price 6d. net; (iii) A General Mathematical Syllabus for Non-Specialists in Public Schools, price 2d. net. These reports may be obtained from the Publishers of the *Gazette*.

(iv) **Catalogue of current Mathematical Journals**, with the names of the Libraries where they may be found. Pp. 40. Price, 2s. 6d. net.

"Even a superficial study convinces the reader of the general completeness of the catalogue, and of the marvellous care and labour which have gone to its compilation."—*Science Progress*, Jan. 1916.

(v) **Report of the Girls' Schools Committee, 1916**: Elementary Mathematics in Girls' Schools. Pp. 26. 1s. net.

(vi) **Report on the Teaching of Mechanics, 1918** (*Mathematical Gazette*, No. 137). 1s. 6d. net.

(vii) **Report on the Teaching of Mathematics in Public and Secondary Schools, 1919** (*Mathematical Gazette*, No. 143). 2s. net.

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VOL. X. JULY, 1920. No. 147.

THE LEEDS MEETING.

By R. C. FAWDRY, M.A., B.Sc., Head of the Military and Engineering Side, Clifton College.

THE hospitality of Yorkshire is a tradition of venerable antiquity—add to this the invigorating effect of the moorland air, which apparently infects not only the natives but also the aliens who take up their abode in the County, and we clearly have a combination which augured well for the success of the meeting at Leeds.

The event must have proved especially gratifying to Professor Milne, who, after enrolling 100 members in the new Yorkshire branch, made himself responsible for the organisation of the proceedings, and was rewarded by an attendance varying from 80 to 120.

Rarely has the annual meeting in London attracted so many eminent mathematicians to its deliberations. The President, Professor Whittaker, came from Edinburgh to preside, Professor Gray, Lord Kelvin's successor, came from Glasgow, and there were also present Sir George Greenhill, Professor Leahy from Sheffield, Professor Heawood of Durham, and Professor Neville from Reading, besides Dr. Milne and Dr. Brodetsky of Leeds. Arrangements for the entertainment of visitors from afar were made by the energetic secretary of the Branch, Rev. A. V. Billen, Head of the Mathematical Department of Leeds Grammar School.

The President's address was an amplification of the subject to which he referred at London in January, and dealt in more detail with the problems in connection with Curve Smoothing and Curve Fitting, a branch of research work on which his students are engaged at Edinburgh. The relative merits of the formulae obtained by series, mean squares and probability were discussed and illustrated. The application of the formulae to a series of readings taken from 4-figure logarithm tables to bring them on a curve constructed from 10-figure tables was an ingenious test of their value, and it was shown possible to obtain the fifth figure by this method without any knowledge of logarithms at all.

The reception by the Vice-Chancellor, Sir Michael Sadler, was greatly appreciated as a means of making the acquaintance of our hosts, the Council and Senate of the University, and of becoming better known to one another. At the dinner which followed, the Vice-Chancellor in a felicitous speech gave a hearty welcome to the Association, and Professor Whittaker in his reply emphasised the value of the work done by the Association by keeping the Universities

in touch with the teachers. Members were then invited to the Great Hall to hear a lecture by the Vice-Chancellor on the new movement in painting with reference chiefly to the works of Cézanne, Ganguin and Van Gogh. He showed to what extent the methods of these artists had been practised in sketches by Rembrandt, Constable and others, and his enthusiasm would have gone far to convert the Philistines had not the absence of colour in the slides deprived the pictures of their chief charm and emphasised the indifference to drawing shown by most of the painters.

Tuesday morning opened with a tour of the Textile Department under the guidance of Professor Barker. The various stages in the preparation of wool, culminating in its use in the weaving of cloth and carpets, were full of interest, and many regretted that the time allotted was too brief for a more detailed inspection.

The processes in the treatment of leather were explained by Professor M'Candlish, and a visit was made to Professor Proctor in his research laboratory. Great interest was evolved in the Leather Department by the exhibition of a machine for measuring the area of leather by means of a mechanical application of Simpson's Rule.

The discussion on the training of the mathematical teacher was opened by Professor Milne, who wished the course to include a study of the History of Mathematical Methods and Teaching. Professor Strong urged the necessity for extending the scope of the training, and outlined a scheme of great breadth which it would seem impossible to cover in the time allotted. This was followed by an interesting controversy with Professor Whittaker, who condemned the Scotch system, which insisted on a year's training, and thus made it impossible for students to engage in research and at the same time to become qualified as teachers.

An interesting feature of Professor Barker's lecture on the mathematics of thread and cloth construction was an illustration of the nomographs which Dr. Brodetsky has constructed for the use of the Textile Department, where they have proved savers of time and money.

Dr. S. A. Shorter demonstrated the mathematical laws which underlie the process of weaving sateens, and Mr. Whitwam pleaded for the abolition of unnecessary formulae in text-books used by technical students, in favour of an intelligent knowledge of the underlying principles.

Readers of the *Gazette* will have the pleasure of reading Professor Gray's paper on Dynamics in full. It proved to be of such absorbing interest that the meeting insisted on his consent being given to its publication.

The meeting closed with a lecture by Mr. A. N. Shimmin, in the absence of Professor Jones, on Mathematics and Commerce. Mr. Shimmin outlined the general problem in commerce, and showed where the Mathematician could come to the aid of the Economist. He described a very interesting syllabus of the mathematical needs of the commerce student.

This brief resumé will perhaps be sufficient to show that the experiment of holding a meeting of the Association outside London was highly successful, and that a high standard has been set for any future efforts of a similar kind.

Many teachers inclined to find their daily task dull and monotonous must have felt inspired afresh, not only by contact with men of vigorous personality and wide outlook, but also by the evidence of the importance of their work in its bearings upon many branches of modern industrial operations.

In conclusion, it must be noted that Leeds University seemed inadequately housed considering the valuable nature of the work it is doing, and it is unfortunate that amongst the many wealthy merchants of Yorkshire no one is found to rival in munificence gifts such as those made by Mr. Wills to Bristol University.

R. C. F.

THE TRAINING OF THE MATHEMATICAL TEACHER.

By WILLIAM P. MILNE, M.A., D.Sc., Professor of Mathematics, University of Leeds.

THE professional training of the Mathematical Teacher after his degree course is finished is a principle which has come to stay, and it therefore behoves all who are concerned in the practical administration of this principle to do their utmost to make the course of professional training as valuable as possible for the future teacher. So far as I know nothing has as yet been done by the Mathematical Association as a body in helping forward this movement, but surely the Mathematical Association will fail in its fundamental aims if it does not take a very active part in helping to decide what are the best courses of instruction to give to the student-teachers while in training. The object of the present paper is not to lay down dogmatic assertions as to what ought to be done, but rather to put forward suggestions, and thus to act as a nucleus round which the discussion can revolve. No attempt is made to deal with the training of anyone except the specialist type of mathematical teacher,—the person who in time to come is destined to teach in the Advanced Departments of the Secondary Schools in addition to taking his share in the general mathematical work of the school, and who, if weighed in the balance and not found wanting, will be called upon to act as Head of a Mathematical Department, and thus to guide and mould the mathematical destinies of the institution to which he is attached. In almost every University nowadays there exists a Department of Education, and within that Department of Education a School of Mathematical Pedagogy. It is for us to discuss now what ought to be the type and scope of instruction given in this School of Mathematical Pedagogy.

The normal course of study in the School of Mathematical Pedagogy is one year, and I would suggest first of all that during this year the student be not cut off entirely from his general mathematical interests. Even two hours a week devoted to post-graduate study of some branch of advanced mathematics in which he is particularly interested will keep him fresh and not allow him to rust, mathematically speaking. It will also be a welcome change from educational psychology and class-room practice. The man is to be a teacher of mathematics, and the first essential of a teacher is to have something to teach. He must not be allowed to forget the technical knowledge of his craft.

The next point to consider is, that in order that the future teacher may have a proper understanding of the methods of teaching mathematics to-day, he must have a sound historical knowledge of the development of mathematical teaching in the past. This requires both a knowledge of the history of the progress of Mathematics regarded as a science,—a subject which has been too much neglected by Universities in the past,—and it also requires a detailed study of the past methods of giving instruction in Mathematics. Thanks to the labours of Sir T. L. Heath and his scholarly editions of the works of the Greek Mathematical Teachers, it is possible to reconstruct the ancient methods of mathematical instruction in the Greek world and to picture their schools of study. The traditional method of propagating new theorems among the old Greeks is also of interest to young teachers,—how the ancient Greek scholar interested both in abstract things and in the selling of his merchandise often cruised about among the islands vending his wares and discussing new mathematical theorems. Then comes the Mathematical World of the Middle Ages and the Renaissance. Take for example the views of Roger Bacon, who flourished about 1300, on the teaching of Mathematics. He strove hard to replace logic in the University curriculum by mathematical and linguistic studies, and it may cheer many a non-mathematician to know that his view that "divine mathematics alone can purge the intellect and fit

the student for the acquirement of all knowledge" fell on deaf ears in his own day.

All through these centuries there is the record—clear here and dim there—not only of how Mathematics was taught, but also of the position of the subject in a liberal education. A knowledge of this important branch of history is surely essential to the future teacher in connection with his life's work, if he is to be an intelligent craftsman and not merely a dull purveyor of well-known mathematical facts. Lastly, there is the great movement of reform in the teaching of Mathematics in the present day. Sir Isaac Newton is not usually regarded as a man who would have been an ideal schoolmaster, but his view of an efficient Mathematical Department in a properly equipped Secondary School—drawn up for Christ's Hospital—deserves a prominent position on the walls of every school. It is a vision of a school mathematical department—at once idealistic and practical—that is but rarely realised even at the present day. The history of mathematical teaching in Great Britain from earliest times is but little known. A certain amount is known of the syllabuses of knowledge required of pupils in bygone days in some of the great and ancient English schools, but scholarly research on the whole subject has not yet been undertaken. Surely this is a good historical subject for some graduate in the Department of Mathematical Pedagogy of a University who wishes to qualify for his Ph.D. degree. Enough has been said, however, to show that the training of the future mathematical teacher is incomplete if he does not possess a sound historical knowledge of the history and development of the Methods of Mathematical Teaching and the place of Mathematics in a Liberal Education from earliest times to the present day.

We come now to the more technical aspect of the young teacher's training. In his school of Mathematical Pedagogy he will naturally ask for guidance and advice in his future teaching of such subjects as Ratio and Proportion in Geometry, Theory of Parallels, the fundamental theorems of the Calculus and so forth. A rigorous study of these subjects is impossible for young boys, but they can and do profit by a course of geometrical study in which intuition is allowed to play an important part. The question arises therefore, "Where does intuition stop and rigour begin?" The teacher is being continually forced to face this question and to face it honestly. Take for example the question of Ratio and Proportion—the crowning glory of Greek Geometry. Boys cannot face rigorous proofs based on logical definition and deduction, but they ought not to be put off with a welter of rigorous logic here, sheer intuition there and proofs that are vicious elsewhere. The teacher must himself understand his ground before he is in a position to give satisfactory presentation of the subject to boys. To do so, he must have made a profound study of Ancient Greek Geometry, of the attempts through all the centuries to get at a simpler explanation of the Greek treatment of ratio and proportion, of the work of Dedekind, etc., on the modern conceptions of measurement, the correspondence between numbers and points on a line, and so forth. Then and only then is the teacher in a position to know where he stands himself, and therefrom to devise a simple treatment of the subject for boys in which intuition and rigorous logic play each a legitimate but definitely assigned rôle. Take again the theory of Parallels. The teacher must have a real knowledge of the inherent difficulties of this problem and not merely a vague notion that there are difficulties somewhere, which are best overcome by using theorems got from some printed book. The teacher must know where he stands, and to do so he must have studied at his school of Mathematical Pedagogy the Greek Geometry of Parallels and the later historical researches of Lobachefsky and Bolyai on Non-Euclidean Geometry. He will then be in a scientific position to disentangle Logic and Intuition and so to devise a treatment suitable for his boys, where they will have nothing to unlearn later on. Finally, consider the Calculus. Much print and more discussion have been wasted at pedagogic conferences in trying to give a

suitable presentation of the "limit" in finding a differential coefficient than in almost any other subject except Congruence Theorems in Geometry. I can offer no solution, but I do feel that we ought to save the young teachers of the future from many of our avoidable errors of the past by insisting that during their course of training they should be equipped with the necessary scientific knowledge and outlook without which no satisfactory solution is possible. For this is necessary a profound study of the elements of Modern Analysis.

It may be argued that all this ought to be known as part of the University Course of Mathematics and that all the School of Mathematical Pedagogy has to do is to superpose the clinical experience and the "bedside manner." But this is not so. The object of a University Course in Mathematics is not to turn out a technical expert in some of the applications of mathematics, such as mathematical teaching, but to give the student a general training in the subject of mathematics, and the branches chosen for the student will depend partly on the taste and special studies of the University Lecturers and partly on those of the student himself. The course at the Training College has, however, a specific end in view, namely to equip the teacher-in-training with those precise branches of mathematics which are necessary for a complete understanding of what he has to teach in a Secondary School and to correlate the science of mathematics with the science of psychology, and both with the human material at the various ages of life at a Secondary School. It is a specialised subject, and requires lecturers who have made a special study of these aspects of mathematical, psychological and educational study.

I would suggest in conclusion, therefore, that the course of training at the School of Mathematical Pedagogy to which the student is attached should comprise:

I. Clinical Mathematics, that is, teaching practice in the Classroom.

II. The History of Mathematics and Mathematical Teaching, with special reference to Ancient Greece and Mediaeval and Modern England.

III. Those more philosophical branches of Mathematics which are essential to an intelligent understanding of the inherent difficulties underlying those parts of Mathematics taught in a Secondary School.

IV. Post-graduate lectures on Mathematics, or research, for two or three hours a week.

If the course is less complete than this, our students have asked us for bread and we have given them a stone. W. P. M.

GLEANINGS FAR AND NEAR.

49. What poet would essay
To . . .
. . . . make arithmetic and epic meet,
And Newton's thoughts in Dryden's style repeat ?

Man does with dangerous curiosity
These unfathom'd wonders try:
With fancied rules and arbitrary laws
Matter and motion he restrains;
And studied lines and fictitious circles draws:
Then with imagin'd sovereignty
Lord of his new hypothesis he reigns
. . . . . till some usurper rise;
And he too, mighty thoughtful, mighty wise,
Studies new lines, and other circles feigns.

—Prior, *An Ode*, 1688, written as an Exercise at St. John's Coll., Cam.

THE MATHEMATICS OF THREAD AND CLOTH CONSTRUCTION: AN HISTORICAL SURVEY.

By Professor A. F. BARKER, M.Sc., Head of the Department of Textile Industries, University of Leeds.

As a student in the Department of Textile Industries of the University of Leeds—then the Yorkshire College—I was taught to emulate the successes of my forefathers in the spinning of yarns and in the weaving of fabrics,—and also to perpetuate some of their errors. Empirical work, without any discrimination or "imaginative insight," was the fundamental basis of practically all instruction.

Suggestions had already been thrown out by Beaumont, Murphy, Alçan and others that cloths might be built on scientific lines, but until about 1890 all such suggestions were regarded in this country as the dreams of theorists—and as dreams absolutely futile. Between 1880 and 1890 the late Mr. T. R. Ashenhurst, then of the Bradford Technical College, worked out by direct measurement in some cases and by reasonable inference in other cases, the diameters of woollen, worsted, cotton, silk, etc., yarns, and immediately following the publication of these results and deductions came the rule that

$$\sqrt{\text{yds. per lb.}} = \text{reciprocal of yarn diameter in fractions of an inch}$$

$$(20 \text{ sk. wl.} = \sqrt{20 \times 256} = 60, \text{ or yarn has dia. of } 1/60'');$$

a rule which has since been shown to be the accidental result of taking all the factors of yarn diameter into account—specific gravity of wool, relationship of area of cross-section and length to volume, etc., etc.

Almost immediately following this rule came Ashenhurst's trigonometrical investigations into cloth structures, in which the "angle of curvature" played such an important part.

These two researches together initiated a complete change of attitude on the part of the textile designers to theories of cloth construction; and later years have seen the basic principles, then evolved, practically applied, corrected and re-corrected, until to-day almost all cloth structures are referred to these basic principles.

From 1890 to the present day the tendency has been to accept the possibility of basic rules, which must be qualified by certain factors—empirically ascertained—for kind of fibre, quality of fibre, type of thread structure, type of cloth structure, etc., etc.

It will perhaps clear the ground here to state that in all the confusion of ideas brought forward by later workers in this field, there have really been but two lines of investigation pursued:

First, What were the possibilities of cloth structures geometrically and trigonometrically defined, taking yarns as perfect cylinders, easily bendable but incompressible; and

Second, What variation from these theoretical possibilities would be observable due to (*a*) variations in the thread structure; (*b*) variations in the interlacing or cloth structure following the varying flexibility of the threads employed incident upon the weave floats; and (*c*) variations due to "finish"?

My own method has been to teach rigidly and exactly the principles underlying the interlacing of threads regarded as perfect cylinders (Fig. 1), and then upon this exactitude to impose the variations empirically ascertained.

But even this apparently straightforward method is not perfect. For example, if threads are flexible in one direction, why not in another direction—why should they not bend in any and every direction? (Fig. 2). How can we even theoretically perfectly represent interlacings?

It has taken ten years to make cloth constructors realise that in such a simple structure as $\frac{2}{2}$ twill, not only do the threads bend in a direction vertical to the plane of the cloth, but that they also bend in the cloth plane. In

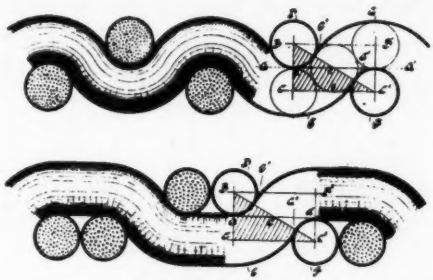

FIG. 1.—From *Textile Design*, Methuen & Co.

passing, it may be remarked that the suggestion given by this recently observed bending is very suggestive to the textile designer—gauze interlacings, for example, being based upon accentuations of the bending here noted (Fig. 2).

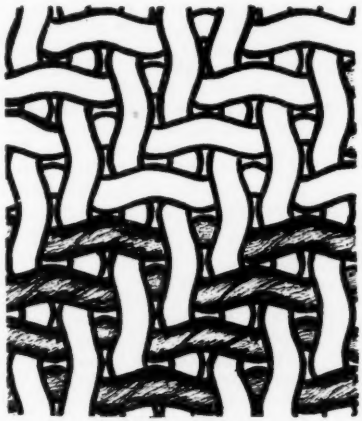

FIG. 2.

It is here interesting to note that from among the workers engaged on the problems referred to, Professor Bradbury of Belfast has principally worked on the theories of interlacing, Mr. Armitage of Huddersfield Technical College has investigated empirically the variations incident upon the natures of woollen and worsted threads, and Mr. Law of Leeds University has experimentally investigated variations in structure incident upon length and relationship of weave float. The final result is that we have now a mathematical basis for the construction of standard cloths, and, better still, that imaginative insight into possible variations which enables us to bring forth new creations.

The success which followed the researches of Mr. Ashenhurst and the work of other researchers into cloth construction naturally suggested that thread

construction might also be similarly investigated with advantage. Such investigations have been carried out fairly extensively since 1892.

The development of thread construction theories may be best illustrated by considering relative twist in yarns. Most writers on yarns and cloths realised

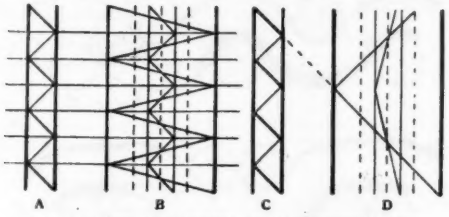

FIG. 2.—From *Cloth Analysis*, Scott, Greenwood & Sons.

that twist might be defined as angular variation from the longitudinal direction of the thread of the fibres composing the thread, and that if this method of indication were adopted it would be necessary to define twist in terms of the diameter, *i.e.* in proportion to the $\sqrt{\text{counts}}$. Thus the "stupid" unimaginative rule that "twist varies inversely as the square of the counts" was put forth. It rested with me personally to show what ought to have been at once apparent, viz., that if the supposed exterior angle of twist is retained the interior structure of counts of different number must be different, and conversely that if the interior structure of the threads were the same the exterior fibre arrangements or twist angles would be different—the two things are incompatible (Fig. 3). It has rested with Mr. Henry Wilkinson of Wyke to show that if any given quality of material (really diameter of fibre) be taken to have a numerical value equal to

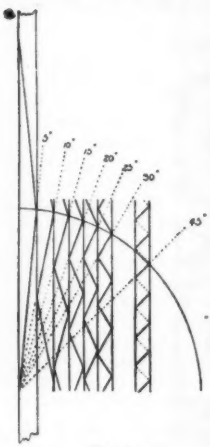

FIG. 4.

$$\frac{1}{q1{\cdot}52^{1\cdot 6}},$$

in which q = quality number, then it is possible to reconcile the apparently irreconcilables, and hence it may be possible within certain limits to make relative changes which are fairly exact.

The emphasis continually placed by cloth constructors upon "angle of twist" in the yarns employed, led me to the conclusion about ten years ago that twist in yarns should preferably be determined by angle of twist with the longitudinal direction of the thread (Fig. 4). Spinners, however, objected to this definition, contending that turns per 1″ was much the simpler method of definition. To overcome this objection I had a graph on ordinary squared paper prepared, from which the turns per 1″ might be read off for any count of yarn and for any angle (Fig. 5). The curves, it will be observed, present considerable variation from one another, and the graph is much more difficult to draw and read than the simple straight-line graph; but on the suggestion of a member of the engineering staff (Mr. Stelfix) I had these curves plotted out on logarithmic paper (Fig. 6), when straight lines, much more easily to be read, resulted. But better still, Dr. Brodetsky has worked these factors into a "Nomogram" (Fig. 7), which, drawn on a larger scale, seems completely to overcome any practical difficulties in defining twist by "angle" rather than by "turns per inch."

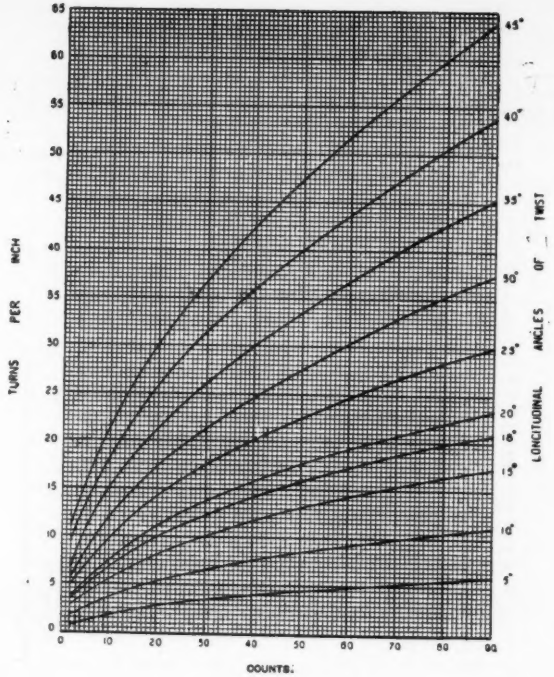
TURNS PER INCH
LONGITUDINAL ANGLES OF TWIST
COUNTS.

FIG. 5.

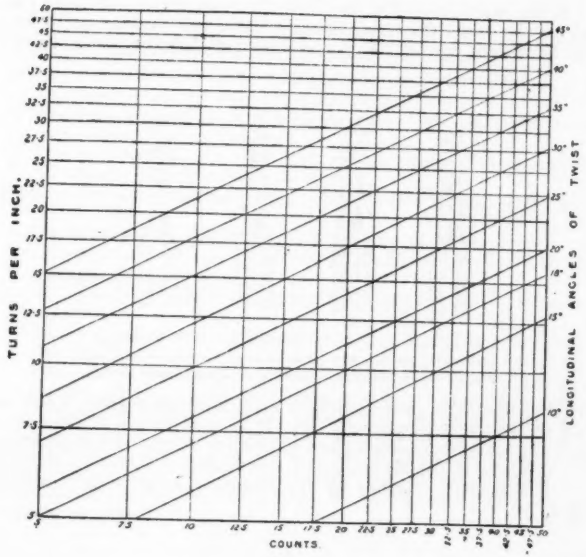
TURNS PER INCH.
LONGITUDINAL ANGLES OF TWIST
COUNTS.

FIG. 6.

Following up the idea of "angle of twist," it was soon found necessary to differentiate between "fibre angle" in the case of single yarns, and "twist

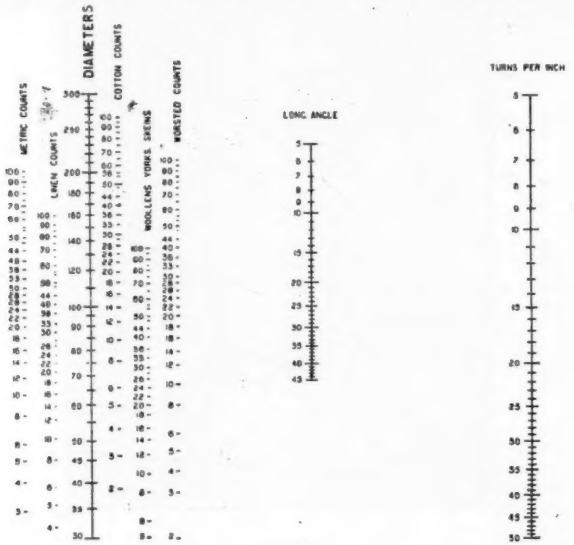

FIG. 7.

angle" in the case of folded yarns. The six types of two-fold twist yarns, as defined by the terms Converse, Straight fibre, and Concurrent twists, thus came to be worked out (Fig. 8). The turns per 1″ for any count of yarn for

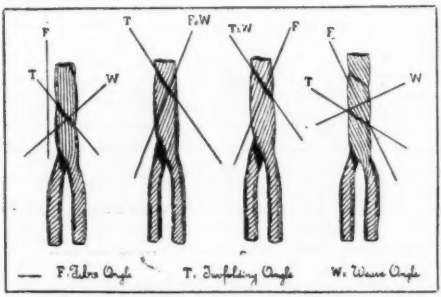

FIG. 8.—From the *Journal of the Leeds Unity Textile Society.*

any required condition have thus been made a simple matter, which can be read off from such a Nomograph (Fig. 7) as that provided. The necessity for the yarn constructor employing imaginative insight is well illustrated in Fig 9. Members of the Mathematical Association will also be interested in other "Nomograms" designed by Dr. Brodetsky to solve particular textile problems.

But the greatest difficulty with reference to scientific work in cloth and thread structure must now be considered. This arises from the many units of measurement employed in the textile industries. Leeds, Bradford, Huddersfield, Manchester, Glasgow—all these textile centres each speak a special textile language, and each has peculiar units of number, length, weight, etc.

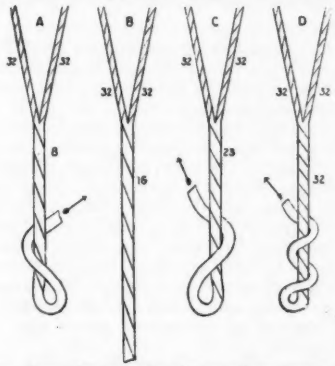

FIG. 9.—From the *Journal of the Leeds Unity Textile Society*.

The numbers in the diagram represent "turns per inch" in the single and twofold yarns. With the first conditions (left) the single twist dominates and causes the twofold yarn to twist upon itself as shown. With the second conditions single twist and twofold twist balance one another, and with the third conditions the twofold twist dominates.

With the addition of experimental laboratories to our textile factories the metric system must also be included, and it is to be hoped that ultimately the metric system will be adopted not only in the laboratory but throughout the industry.

Perhaps, in conclusion, one example illustrating the numerical difficulties involved in mounting a loom to weave a figured fabric may be given:

(*a*) The count of yarn (20s, 30s, 40s, etc.) may be stated on a group unit basis of 256 or 560 or 840, etc., etc.

(*b*) The threads may be stated in group-units of 38 or 40 or 42 threads—a relic of counting by the score.

(*c*) The weight of the warp and weft may be stated in warterns (6 lbs.), *i.e.* quarterns of the old 24 lb. stone.

(*d*) The length of the warp in the Leeds district will be given in "strings" of 10 feet.

(*e*) The heald shafts will group the threads in 4s, or 6s, or 8s for Tappet looms, in any grouping up to 36 for Dobby looms, or up to 1200 for Jacquard looms.

(*f*) The colour pattern of the warp may be grouped in anything from 2 to say 200.

(*g*) The reed will group its threads in 2, 3, 4, 5, 6, 7s, or 8s; and more important still, the (*e*), (*f*) and (*g*) groupings must be exactly adjusted the one to the other. Anyone having these varying groupings to adjust—the one to the other—will realise what an iniquitous imposition the multiplication table as ordinarily taught in our schools is—how it enervates and hides rather than stimulates and reveals.

Thus it will be evident that thread and cloth construction are no "clerking job," but specially require that imaginative insight which surely leads to intelligent manipulation and control of all the factors involved in the solution of the problems presented.

A. F. B.

THE MATHEMATICAL THEORY OF THE SATEEN ARRANGEMENT.

By S. A. SHORTER, D.Sc., late Member of the Research Staff in Textile Industries, University of Leeds.

GEOMETRICALLY considered the sateen arrangement is a certain regular method of arranging points on squared paper, each point being placed at the centre of a square. In textile work the sateen arrangement is used, (1) as a basis for actual weaves, (2) as a mode of distributing over the surface of a cloth the repeats of an ornamental design.

We may regard the sateen arrangement as being generated in the following manner. Let us place on squared paper a point A in a certain square * (see Fig. 1), then a point B in the next column of squares, a certain number of, say two, squares higher, a point C in the next column the same number of squares higher than B, and so on. We thus obtain what is known as a "twill" or "diagonal." Now suppose that by a thickening of some of the lines of the squared paper, the paper is divided into larger squares whose sides are, say, five times as long as those of the small squares. Now suppose that the arrangement is one which repeats itself in each of the large squares. Placing D' and E' in the large square containing A to correspond with D and E, we obtain one complete element of the pattern which may be repeated *ad lib.* over the rest of the paper.

In the above case I have chosen 5 as the number of times the side of the small square is contained in that of the large square, and 2 as the number of squares the "twill" ascends from one column to the other. The resulting design is a sateen of order 5 (or a 5-end sateen) with a step of 2 (or stepping 2). These two numbers, the "order" and the "step," completely specify a sateen. We may not, however, choose any two numbers. In order to obtain a weavable design we must put a point in every row and every column of the design square. Thus, if we make the twill of Fig. 1 repeat on a design square of 6, we obtain the result shown in Fig. 2, which does not fulfil this condition.

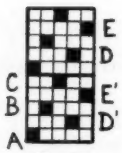

FIG. 1.

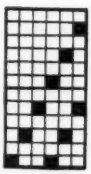

FIG. 2.

FIG. 3.

This is owing to the fact that the points begin to repeat on the rows of the design square before all the columns have been filled. In order that this may not happen the L.C.M. of the order and step must be equal to their product. Hence the first well-known sateen rule—the order and the step must be prime to each other.

There is a second well-known rule which relates to the lateral inversion of the design. Let A, B and B' be three points in a sateen arrangement (see Fig. 3), B and B' being in a column adjacent to A, and B' being an adjacent repeat of B so that the distance BB' is equal to the order of the sateen. Now we may suppose the sateen to have been generated (a) by stepping in the direction AB (5 squares upwards to the right), (b) by stepping in the direction

* In practice these "points" are expanded into solid black squares.

AB' (3 squares downwards to the right). If a step of this latter magnitude were taken in the same direction as the former step, we should evidently obtain the mirror image of the sateen. Moreover, the sum of the steps is equal to the order of the sateen. Such steps are said to be complementary. We thus see that complementary steps give sateens which are the mirror images of each other. This is the second well-known sateen rule. In Fig. 4 is shown

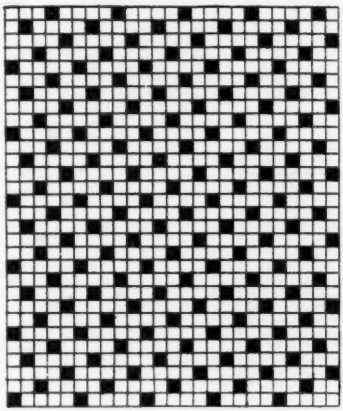

FIG. 4.

the 5/3 sateen,* which, in accordance with this rule, is the mirror image of the 5/2 sateen shown in Fig. 1.

These two rules seem to comprise the whole of the hitherto published systematic knowledge of the subject. There is in existence no general theory of sateens, in spite of the importance of the subject in textile designing. The object of the present paper is to formulate a mathematical theory of the subject and arrive at the general principles underlying those properties of sateens which are of importance in designing.

The first important question is that of the number of essentially distinct sateens of any given order. The first rule limits the possible values of the step to numbers prime to the order. The second rule shows that only half of these possible sateens are essentially distinct designs. There is, however, another limitation. It is evident that the rotation of a sateen through a right angle will in general give another sateen with a different step (and, of course, of the same order). The relation between the steps of two sateens derivable one from the other by a rotation through a right angle is readily investigated. In Fig. 5 the generating twill of a sateen is continued till we reach a point H in the top row of a design square, *i.e.* one row below a repeat (A'') of the initial point A. Now we may regard the sateen as being generated by stepping in the direction $A''H$. A rotation through a right angle converts this into the normal "upward to the right" mode of stepping. Now it is evident that the product of the new step and the old is less by unity than a multiple of the order. Hence the condition for two steps s_1 and s_2 to give sateens derivable one from the other by a right angle rotation is that

$$s_1 s_2 = in - 1,$$

where i is an integer.

* For the sake of brevity we will call the sateen of order n and step s the n/s sateen.

A consideration of the step $A'E$ (Fig. 5) will readily show that if the condition

$$s_1 s_2 = in + 1$$

is fulfilled, the rotation of one sateen gives the mirror image of the other.

We thus have a third rule limiting the number of essentially distinct patterns. An application of the three rules shows that even for high orders the number of patterns is in general small. For example, consider sateens of order 20. Applying the first rule we obtain the possible steps *

3, 7, 9, 11, 13, 17.

We may cut out the second three as yielding mirror images of the first three; and since $3 \times 7 = 20 + 1$, we may cut out the 7. We thus have only two distinct patterns given by the 3 and the 9 (or by the 7 and 11, 13 and 9, etc.).

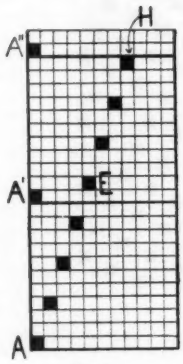

FIG. 5.

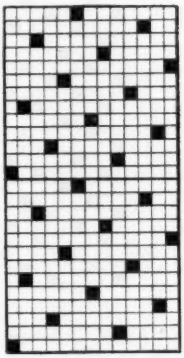

FIG. 6.

We obviously have the best chance of obtaining a large number of patterns when the order is a prime number. Thus in the case of the order 13, the steps

2, 3, 4, 5, 6,

left by the application of the second rule, may be reduced to

2, 3, 5

by the application of the third rule, so that even in this case there are only 3 distinct patterns. It will be seen that the 2 cuts out the 6 and the 3 the 4. The step 5 is peculiar; we have $5^2 = 2 \times 13 - 1$. Hence the 13/5 sateen is unaffected by a rotation through a right angle. This sateen is shown in Fig. 6. It will be noticed that the points lie at the intersections of a square lattice, which, as can be shown from elementary considerations, is not altered in appearance by rotation through a right angle.

We will not consider further the question of the number of sateens of any given order, but will proceed to the consideration of what may be termed the "sateen lattice." We may regard the points of a sateen arrangement as lying at the intersections of two series of equidistant parallel lines. This "sateen lattice" is not unique for any given sateen. Every sateen may be regarded as giving rise to an infinity of lattices, though the number actually suggested to the eye by any sateen is limited. Thus in the 17/10 sateen indicated in Fig. 7, the elementary parallelograms of two possible lattices are shown.

* The step of one, or one less than the order, gives a continuous twill which is not regarded as a sateen.

In distributing an ornamental figure on a sateen basis, the shape, size and orientation of such parallelograms are obviously of importance in relation to the shape, size and orientation of the figure. It may perhaps not be out of place to point out at this stage the characteristic virtue of the sateen distribution—not possessed by simpler arrangements (such as the " half-drop "

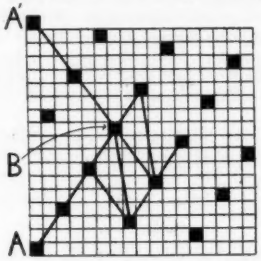

FIG. 7.

arrangement, much used in the design of wallpapers, which is founded on a lattice arrangement whose elementary equilateral parallelogram has its two diagonals respectively horizontal and vertical). A sateen arrangement which possesses a square or nearly square lattice does not obtrude its regularity on the eye, and may even produce a pleasing effect of irregularity.

Not every lattice arrangement gives rise to a sateen. If we suppose the lattice to be specified in shape and size of mesh and orientation by the four measurements a, b, c, d indicated in Fig. 8, it is necessary that a should be

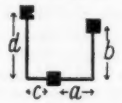

FIG. 8.

prime to c, and b to d. This follows from elementary considerations. We may thus generate a sateen from an elementary parallelogram specified by any four numbers, a, b, c, d fulfilling this condition. The following problem immediately suggests itself. What is the relation between the order n and step s on the one hand, and the four numbers a, b, c, d ? A purely empirical way of solving the problem in a concrete case would be to fill in the design according to the specified plan until two points were obtained, (a) in the same column, (b) in adjacent columns. The vertical distance between the points would give in case (a) the order, and in case (b) the step (or its complement). A systematic method of obtaining the order is shown in Fig. 7. Let us start from a point A along an " upwards to the right " lattice line till we arrive at a point B whose horizontal distance from the starting-point, which is obviously a multiple of a, is also a multiple of c. Since a and c are prime to each other, this distance must be ac. If now we proceed back towards the initial column along an " upwards to the left " lattice line, we shall obtain a point A' vertically above A. The first part of the journey consists of c stages, each with a vertical component b, and the second part of a stages each with a vertical component d. Hence the total vertical component is $bc+ad$. Hence we have for the order,

$$n = ad + bc$$

or

$$n = \begin{vmatrix} a & b \\ -c & d \end{vmatrix},$$

which is a very simple relation between the order and the rectangular coordinates of the diametrically opposite corners of an elementary parallelogram relative to one of the remaining corners.

For the same sateen we may have an infinity of sets of values of a, b, c, d, which should give, according to the above formula, the same value of n. If we consider the series of parallelograms on the same base and between the same parallel lattice lines (two consecutive members of which are shown in Fig. 7), we see that the passage from one member to the next corresponds to the addition of members of one row of the determinant to the corresponding members of the other row, a process which does not alter the value of the determinant.

The value of the step may be determined in similar manner. If h stages upwards to the right and k stages upwards to the left terminate at a point in a column immediately to the right of that occupied by the initial point, we have

$$ha - kc = 1.$$

The step is given by

$$s = hb + kd.$$

Eliminating k, and making use of the formula for the order, we obtain the relation

$$s = \frac{hn - d}{c},$$

in which h is the least integer which will give an integral value for s.

In Fig. 7, for the "squarer" of the two lattices indicated, a, b, c and d have respectively the values 2, 3, 3 and 4. Our formula for the order gives the value 17. For the step we have

$$s = \frac{17h - 4}{3},$$

which for $h = 2$ gives $s = 10$. Hence the sateen is of order 17 and step 10.

We will conclude by considering one more question of the many arising out of a consideration of the sateen arrangement. This question relates to what may be termed the "twilliness" of a sateen. Certain sateens suggest to the eye a definite "twill," while others are devoid of any such suggestion. For most purposes for which the sateen arrangement is used in textile work this suggestion is a defect, though for certain special effects a marked "twilliness" may be desirable. A comparison of the 17/2 sateen shown in Fig. 9 with the 5/3 sateen shown in Fig. 4 will reveal the origin of "twilliness." If

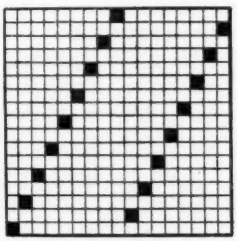

FIG. 9.

the most nearly rectangular parallelogram has very unequal pairs of sides, then there is a marked twill in the direction of the shorter sides, the nearness of the points suggesting to the eye a continuous line. If the sateen yields a square lattice, then we get a minimum of "twilliness." Such sateens are generally called "perfect" sateens, since they possess in the most marked

degree that combination of the regular and the chaotic which was the primary object of the invention of the sateen arrangement.

The condition for a square sateen is evidently that d and c should be respectively equal to a and b. Hence we have

$$n = a^2 + b^2,$$

i.e. the order of a square sateen is equal to the sum of the squares of two numbers which are prime to each other. The step may be deduced from the relation

$$s^2 = in - 1,$$

which expresses the fact that the sateen is not altered by rotation through a right angle. The smallest value of i giving an integral value of s, gives the smaller of the two complementary steps.

The following table shows the order and step of the first seven "perfect" sateens:

Order.	Step.
$5(1^2+2^2)$	2
$10(1^2+3^2)$	3
$13(2^2+3^2)$	5
$17(1^2+4^2)$	4
$25(3^2+4^2)$	7
$26(1^2+5^2)$	5
$29(2^2+5^2)$	12

In the general case the ratio of the distances between consecutive members of the two sets of lattice lines is evidently

$$\sqrt{\frac{a^2+b^2}{c^2+d^2}}.$$

Hence the deviation from the value unity of this quantity in the case of the most nearly rectangular lattice given by any sateen affords a kind of numerical measure of the "twilliness." *

There are many other problems of a mathematical nature connected with the sateen arrangement, and with textile designing in general, but the present paper will, I hope, be sufficient to show that even so technical a subject as textile designing presents many problems of interest to the pure mathematician.

S. A. S.

* Assuming that a square sateen has "twilliness" of zero, the most suitable measure of the "twilliness" would of course be $\log \sqrt{a^2+b^2}/\sqrt{c^2+d^2}$.

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50. . . . During these graduate years at Pembroke Hall . . . Mr. Pitt laid in his principal stores of knowledge . . . In mathematics, the especial pride of Cambridge, he took great delight. He frequently alluded in later life to the practical advantage he had derived from them, and declared that no portion of his time had been more usefully employed than that which he devoted to this study. He was master of everything usually known by the academic "wranglers," and felt a great desire—but Mr. Pretyman did not think it right to indulge the inclination—to fathom still farther the depths of pure mathematics. "When," adds Mr. Pretyman, "the connection of tutor and pupil was about to cease between us, he expressed a hope that he should find leisure and opportunity to read Newton's *Principia* again with me after some summer circuit."—[George Pretyman, Sen. Wrangler, 1772, tutor of Pembroke Hall, tutor of William Pitt, in 1803 took name of Tomline. Bishop Winchester, 1820; *d.* 1827, *b.* 1750, Bury St. Edmunds.]

THE TEACHING OF MATHEMATICS TO TEXTILE STUDENTS.

By J. H. WHITWAM, B.Sc., Head of the Mathematical and Science Departments of the Holmfirth Secondary School and Technical Institute.

I. Before commencing his purely Textile Course, the student has had, at least, a two years' course consisting of Practical Mathematics and Drawing, English, and Laboratory Work in Mechanics and Physics. The syllabus for Mathematics includes: Mensuration of the rectangle, parallelogram, triangle and circle; surface areas and volumes of the cylinder, cone and sphere; measurements are made from everyday objects and models, and symbolical representation of the results leads to formulae and substitution of numbers in these formulae. From this we pass to simple equations. Squared paper is used to plot results of measurements made (either in the Mathematics lessons or the Laboratory), such as comparing:—circumferences and diameters of circles; areas of triangles of equal altitudes and different bases; extensions and loads for a spring; corresponding readings of temperatures in C.° and F.°, and from these we pass to proportionality, or variation. Powers and roots are dealt with, and the use of Logarithms taught. The Drawing, which is taught alongside the Mathematics, consists of: Hand-sketches, plans, elevations and sections of models and parts of machinery used by the students.

The examples *usually given* are from the Engineering and Building trades, and these are not at all interesting to a student working in a factory and intending later to study Weaving and Designing, Spinning, or Scribbling Engineering. Some 6 or 7 years ago, students of this type, who, being interested in Mathematics, were attending my Mathematics Class for Building Students in addition to their Textile Classes, asked me the question: "How does all this work help us in our textile calculations?"

I suggested that they should bring their textile problems to the Mathematics class. I found that they had note-books full of rules and formulae: one for each type of problem. They were using these mechanically, and they were at once at sea if they met with a problem to which the rule could not be applied directly.

Allow me to illustrate this as clearly and briefly as possible:

(A) There are several systems of numbering or counting yarns, and it is frequently necessary to change from one system to another system.

Suppose we wish to find the cotton counts equivalent to 1/24s worsted counts.

Worsted counts are based on a hank of 560 yards, and the number of these hanks weighing 1 lb. is the count.

The cotton hank is 840 yds. long, and the number of these hanks per lb. is the count.

Then 24s worsted means 24 worsted hanks weigh 1 lb. or (24×560) yards of yarn weigh 1 lb.

$$\left.\begin{matrix}\text{Number of cotton hanks (840 yds.)} \\ \text{in this length of yarn}\end{matrix}\right\} = \frac{24 \times 560}{840};$$

$$\therefore \text{ equivalent cotton counts} = \frac{24 \times 560}{840} = 16\text{s}.$$

To work this and similar calculations various text-books give various rules, thus:

(i) One book gives the following:

If l = yds. in 1 lb. of 1s worsted; l_1 = yds. in 1 lb of 1s cotton.
c = counts of worsted; c_1 = counts of cotton.
y = yds. in 1 lb of yarn; y_1 = yds. in 1 lb. of yarn.

Then $y = l \times c$; $y_1 = l_1 \times c_1$.

But $y_1 = y$; $\therefore\ l_1 \times c_1 = l \times c$.

(ii) Another book gives the rule: "Reduce the given counts to yards and divide by the standard length of the required system."

(iii) Yet another gives the rule:

$$\text{Required counts} = \frac{\text{given counts} \times \text{yds. per hank}}{\text{yds. per hank in required denomination}}.$$

(B) When two or more threads are twisted or folded together it is necessary to find the counts of the twisted or folded thread.

Suppose we wish to find the counts of a compound twist thread composed of one thread of 1/60s worsted, one of 60/2 spun silk and one of 1/30s worsted.

The first step is to find the equivalent worsted counts of the 60/2 spun silk.

Now 60/2 spun silk means that (60×840) yards weigh 1 lb.

The worsted counts are based on a hank of 560 yards.

Hence $$\text{Equivalent worsted counts} = \frac{60 \times 840}{560} = 90\text{s}.$$

For purposes of calculation we may choose to twist any number of hanks of each thread (*e.g.* 1, 2, 30, 60, or x).

Let us twist 180 hanks (560 yds. per hank) of each thread, and if there is no "take up" we shall have 180 hanks of twisted thread.

(C)
Then 180 hanks of 60s worsted weigh 3 lbs.
180 ,, 90s worsted or 60/2 spun silk weigh 2 lbs.
180 ,, 30s worsted weigh 6 lbs.
$\therefore$ 180 ,, twisted thread weigh 11 lbs.
or $\frac{180}{11}$,, ,, ,, ,, 1 lb.;
$\therefore$ counts of the twisted yarn $= \frac{180}{11} = 16\frac{4}{11}$s worsted.

To deal with calculations like this the student is given

(i) a formula: $\frac{1}{c} = \frac{1}{c_1} + \frac{1}{c_2} + \frac{1}{c_3}$, or,

(ii) a rule: "Select the highest count and divide it by itself and each of the given counts; the quotient in each case will then represent the relative weight of each thread in lbs.; then divide the selected count by the sum of these weights and the answer will equal the resultant counts."

Referring to the statement (C) above, it is quite a simple matter to go a step farther, and

(*a*) if given the price per lb. of the various threads we may find the cost of the folded thread, or,

(*b*) we may find the weight of each thread used in producing a certain weight of folded thread.

The rules and formulae are modified to apply to these cases. The same remark holds for problems dealing with "sett" systems or the different methods in vogue of indicating the spacing of the warp threads across the loom. I will not take up your time by giving examples. The point, with regard to all this, I wish to make this morning is, that *a knowledge of the various methods of counting yarns and of the various sett systems in use is all that is really required by the student. Clear thinking and a visualisation of the conditions of the problem leave no need for rules or formulae.*

II. I will now briefly indicate how I have attempted to adapt the Mathematics Syllabus to meet the needs of these students.

(i) Problems dealing with (*a*) the counting of single and folded yarns, (*b*) testing yarns for counts, (*c*) the setting of warps and counting of healds,

(*d*) the weight and cost of warp and weft in woven fabrics, (*e*) wool blending, are purely arithmetical, mainly simple rule of three.

(ii) In finding the weight and cost of the warp and weft required for the making of a piece of cloth to given particulars, expressions like the following require simplifying :

$$\frac{68 \times 38\frac{1}{2} \times 229\frac{1}{2}}{40 \times 840} \text{ lbs.}; \quad £\frac{60 \times 31 \times 48 \times 105 \times 38}{24 \times 560 \times 100 \times 240}; \quad \frac{54 \times 40 \times 64 \times 65}{36 \times 24 \times 560} \text{ lbs.}$$

The student arrives at these expressions from his knowledge of technical terms and simple arithmetic, and a further knowledge of *Logarithms and the Slide-Rule* is most useful, especially as in actual practice the fractions obtained when calculating for a series of similar cloths have a constant factor. Then the log. of this factor may be found or the slide-rule *set* to this value.

(iii) Many problems, such as finding the counts of a thread required to be twisted with one or more threads to produce a twist thread of a given count and cost, are easily worked by *simple equations*. This is preferable to trying to learn rules or formulae. Some problems actually require two unknowns.

(iv) A large variety of textile problems require a knowledge of *Proportionality or Variation*.

(*a*) In the production of a well-balanced cloth, one of the chief factors to consider is the diameter of the yarn employed. An empirical rule is employed for calculating the diameter of a yarn. This is :

$$\text{Diameter of a yarn (in inches)} = \frac{1}{k\sqrt{\text{yards per lb.}}},$$

where k is a constant determined by experience.

The commonly accepted values for k are : 0·85 for woollen yarns, 0·90 for worsted, 0·92 for cotton and linen yarns.

The student is interested to see how this formula has been derived, by considering yarn as a cylinder and assuming the specific gravity of the yarn. (The sp. gr. of yarn is difficult to determine accurately, hence the constant will differ from the value found by experience.)

From the above formula it is easily shown that : "the diameters of yarns of the same material are inversely proportional to the square roots of their counts."

Also the "sett" (or the number of ends which are laid over a standard width) is directly proportional to the square root of the counts.

(*Note.*—Logarithms are extremely useful in these calculations in which square roots are involved.)

(*b*) Establish from first principles the fact that :

"The speeds of pulleys driven by a belt *or* the speeds of gear wheels are inversely proportional to the diameters or numbers of teeth, respectively," and with the aid of a diagram the student will be able to solve the problems dealing with the variation of (1) the speed of the loom ; (2) the speed of the "tappets" for raising the healds ; (3) the "take-up" motion of the loom, *i.e.* winding the woven piece on to the beam at a given rate, so as to obtain the correct number of picks per inch ; (4) the speeds of the various rollers in the "carding engine," the function of which is to separate the wool fibres one from another and arrange them to form one continuous sliver, all parts of which are equal in weight and thickness ; (5) the speeds of the various rollers in the drafting or drawing out of the threads and the speeds of the spindles, etc., in the spinning machines. In all these calculations, constant factors occur, and change wheels are used to vary the speeds.

As an example, let us consider the "take-up" motion.

When the going part moves towards the fabric it presses the catch C, by means of the lever L, to which it is attached by the pin G, forward, thus imparting motion to the ratchet wheel R. Working on the same stud as R

is the change-wheel *W*. This gears with the intermediate *E*, which, through the pinion wheel *D*, imparts motion to the friction-beam roller-wheel *B*. The friction-beam roller *A* turns the piece beam solely by friction. *K* is the holding-up catch.

If the circumference of the beam roller *A* is 13″, the ratchet wheel *R* contains 60 teeth; the intermediate *E* has 120 teeth; the pinion *D* has 20 teeth and the friction-beam wheel *B* has 120 teeth; it is required to find the number of teeth in the change-wheel *W* for weaving 56 picks per inch.

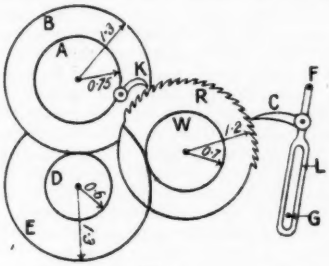

FIG. 1.

When *A* makes 1 revolution, 13 ins. of cloth are drawn on to the beam, and this contains (13×56) picks; hence

in 1 rev. of *A* (56×13) teeth of *R* move forward;

$$\therefore \quad R \text{ makes } \frac{56 \times 13}{60} \text{ revs.}$$

Then (i) $$\frac{\text{speed of } A}{\text{speed of } R} = \frac{60}{56 \times 13}.$$

Assume that the change-wheel *W* has x teeth, and find the velocity ratio of *A* and *R* from dimensions of the gear-wheels, and equate the two values.

Thus (ii) $$\frac{\text{speed of } A}{\text{speed of } R} = \frac{x \times 20}{120 \times 120}.$$

From (i) and (ii), $$\frac{x \times 20}{120 \times 120} = \frac{60}{56 \times 13}; \quad \therefore \quad x = 59.$$

This is a much more preferable method to that of setting the student to work by a rule, thus:

"First, multiply the picks per inch by the circumference of the friction beam in inches, and divide by the number of teeth in the ratchet wheel; second, multiply the teeth in the friction-beam wheel by the teeth in the intermediate, and divide by the first result multiplied by the teeth in the pinion."

(v) *Squared-paper Work.*

(*a*) We may draw graphs to convert the counts of a yarn in one system to the corresponding counts in another system, or to convert settings of warp in one system to the corresponding sett in another system, etc.

(*b*) Use the graphical method in variation problems.

(*c*) Use it in discussing the *motion of the "going-part" of the loom.* The "going-part" serves a twofold purpose. It beats up the weft when in its front position and it provides the medium on which the shuttle may travel from one side to the other when in its back position. The motion communicated to the driving shaft is a continuous circular one, and by means of a crank

and connecting arm, the "going-part" is caused to move backwards and forwards along what is approximately a straight line. This motion must not be regular. At the time the shuttle is passing across the loom, as much rest as possible must be given to the going-part, whilst in bringing it up to the cloth a smart blow must be delivered. Give actual dimensions of the crank radius and length of connecting arm, and let the students make a diagram to scale showing the position of the connecting-pin for each 10° of revolution of the crank. From these measurements draw a displacement-time curve and a velocity-time curve. These show clearly that the velocity of the "going-part" is less as the crank approaches and leaves the back centre, than its velocity as the crank approaches and leaves the front centre. This gives (*a*) the "dwell" necessary to allow the shuttle to cross and (*b*) a smart blow to the cloth by the reed at the time of the "beat-up."

(*d*) Use squared paper in discussing *the motion of the healds.* The healds raise and lower the warp threads, thus making the "shed" through which the weft thread passes. They must be raised gradually at first, with increasing velocity to a maximum, and then decreasing as they reach their highest position. In the tappet loom this motion is imparted by means of tappets or cams which are designed to give harmonic motion. The students construct displacement-time and velocity-time graphs for harmonic motion and also draw designs of tappets for various weaves.

(vi) *Similar Triangles and Trigonometrical Ratios.*

(*a*) The distances through which the warp threads are raised and lowered in making the "shed" for the shuttle depend upon the relative distances moved by various lever-arms. The fact that the corresponding sides of equiangular triangles are proportional helps the tuner of the loom to make the necessary adjustments in the connecting rods.

(*b*) In the simplest weave, each weft thread passes over a warp thread and under the next, and so on, and each warp thread passes over a weft thread and under the next. If the threads lay perfectly straight and the diameter of each warp and weft thread were $\frac{1}{60}''$, then a warp thread and a weft intersection would occupy $\frac{1}{30}''$, and the warp would be "sett" 30 ends per inch. Similarly there would be 30 picks of weft per inch. But the warp and weft threads, when interlacing, bend under and over each other, and the curvature will allow a closer setting of the warp threads and a greater number of picks per inch. The angle due to the curvature affects this.

The figure shows that two diameters occupying $\frac{1}{30}''$, and represented by AB, only occupy a distance represented by AC measured across the cloth, and

$$AC = AB \cos BAC.$$

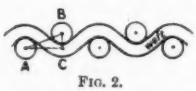

FIG. 2.

There is ample room for coordination between the Mathematical and Textile Departments, along the lines I have suggested to you. *Let the Textile teacher dispense with the majority of his formulae and rules and the Mathematics teacher choose examples from the textile trade.* In this way we can prevent the student thinking that he has entered a new world when he comes to his particular trade calculations, and we may lead him to see that, the builder when estimating quantities and costs of materials; the surveyor when measuring his land; the colliery manager when calculating his ventilation pressure; the bank-clerk when estimating interests and discounts; the textile designer when calculating the weight and cost of cloth, are all simply applying principles learnt in the mathematics class at school.

J. H. W.

ON THE TEACHING AND APPLICATIONS OF DYNAMICS.

By A. Gray, F.R.S., Professor of Natural Philosophy in the University of Glasgow.

I am not going to inflict a sermon on you; but if I had to give out a text for my discourse I should, I think, choose the words, "The letter killeth, the spirit maketh alive." This is a very general truth, but it seems to me to have special force in connection with dynamical science. I shall leave the application to be traced by yourselves as I proceed.

The spirit is the breath of life which pervades not merely certain books and certain writings and sayings of great men, but all true knowledge and all search into the secrets of Nature, whatever the aspect of Nature studied may be. The domain of science is now less restricted than it used to be by boundaries and partition walls bristling with notices to trespassers; all the old divisions into different realms, for example those of mind and matter, in which the philosopher and the physical inquirer respectively wandered, without ever crossing the frontier, were only hindrances to progress.

The letter is that dull formalism which repels the searcher after truth, that wrapping of everything up into formulae and dogmas to be applied mechanically and almost without thought in all circumstances, with which in the region of pedagogics we are all familiar. Sometimes, though not so often in mathematics as in some other sciences, it takes the shape of an undue belief in and deference to authority, in the choice of methods of working and modes of expression, a habit of mind which does not conduce to originality and freshness of treatment. Vector analysts have ranged themselves as followers of Hamilton, as disciples of Grassmann, and as believers in Gibbs and Heaviside, and their disputes have provoked a new version of the caustic comment of Tertullian, in "Behold how these vectorists love one another!"

And I ask also, have we not sometimes an example of the benumbing influence of the letter in that straining after ultra precision of statement and the utmost rigidity of proof which distinguishes some of the mathematical text-books of our time. A book on algebra for example may be exact in all its statements, and rigid in all its proofs, and yet fail to inspire its readers with a desire to explore the secrets of analysis and become mathematicians. I do not decry exactitude, mathematicians must be exact, and if their proofs are logically defective, whose demonstrations will stand? But the attempt to be perfectly exact *all at once*, to include in every statement all the saving clauses and limitations which can be imagined, or which can be justified by the invention of what I may call pathological cases, can only result in discouragement and failure. Even if it has no such catastrophe the literary result is like a well-drawn patent specification, exact, descriptive, and void of possibility of misunderstanding, but hardly inspiring and certainly not poetic. I say "not poetic," for I hold that there is a poetry of a very ethereal sort in mathematics, and there are some great books, for example Fourier's *Analytical Theory of Heat* and Laplace's *Mécanique Céleste*, which are mathematical poems, though there are statements in them which might well make the hair of a precisionist stand on end. Think of the verbiage involved if one were to try to make any ordinary everyday statement absolutely free from possible ambiguity. I am always amused when a Senatus Academicus or similar body in meeting assembled tries, on the spur of the moment, to draft a rule or precept in terms to which no member present can find an objection.

By giving a rapid general view, some of the older text-books and "Systems of Mathematics" exerted a very remarkable influence; they created a love of mathematics in the learner, which the latest products of the ultra-precisionists fail to inspire. And this love once aroused carried the student back again

to encounter the task of confirming his knowledge and making it more precise.

A great deal of course may be done by reading a modern book in the right way, first reading fairly quickly forward to obtain a kind of general view, and then returning and spending a relatively longer time in the careful discussion of details, so that the pupil may have his faith confirmed and his critical faculty fostered. This confirmation is especially essential when ideas occur to a mathematician by an intuitional process. It is true, as Lord Rayleigh said, that an intuitional process may be more demonstrative, especially in physical matters, than strict mathematical proof; but as the history of mathematics shows, it does not do to trust to intuition alone.

This process of reading up and down, and backward and forward, if I may so describe it, is particularly desirable in the study of the differential and integral calculus. If I were advising a student, I should say get a general view of the elementary parts both of differential and of integral calculus as soon as possible, do not spend any very long time practising differentiation and integration of given expressions before going forward to simple applications. Practise differentiation and integration certainly, but do not delay or postpone the study of geometrical and dynamical applications. These will excite interest, and help enormously to produce an appreciation of fundamental principles. Take care to come back, again and again, to a more critical examination of the fundamental ideas. The applications will be found to have taught soundly, though not formally, a great deal of the real inner meaning of the processes, and the abstract discussion will be made much easier by the fact that the learner has a fair stock of more or less concrete ideas. And I think that dynamical illustrations are exceedingly appropriate; though here fundamental difficulties exist which the usual somewhat naive treatment does not discover, and which it is not desirable should be discovered immediately. Dynamical ideas were continually in the mind of Newton when working out his calculus of fluxions, and in them we obtain the best possible illustrations of the notion of a limit. Analytical difficulties also will be cleared up for the physical student by the reading of exactly written special treatises, for example, in the study of Conduction of Heat, the Theory of Waves and Hydrodynamics.

No legitimate source of interest should be disclaimed, and in this connection my own experience may perhaps be worth referring to, though I am afraid it may appear to some a little fantastic. You will observe I am speaking here of mathematical study, while my proper subject is the study of dynamics. But the two things do not really lie apart, no one can study dynamics, and I say further no one can study physics effectively, who does not approach it from the mathematical side.

I began mathematical study in a village school in Scotland a rather long time ago, in fact at the time of the Indian Mutiny. At that time the advanced school books in use in Scotland were of a very different sort from those now in vogue of course, and also from those which came into fashion a little later, which were the somewhat dry and arid, but up to a certain limit sound treatises of Todhunter. There was then no prolonged study of somebody's algebra and trigonometry with so-called higher subjects, such as the summation of series as in Todhunter for example, a most unsatisfactory collection of tricks and dodges, and analytical geometry in Puckle or Todhunter or Charles Smith. Our text-book was a treatise on what was called Practical Mathematics by John Davidson, A.M., Master of the Burgh School of Burntisland. Our schoolmaster, George Johnstone, was not specially a mathematician, but he had been assistant to Davidson, and had beautifully written notebooks containing solutions of Davidson's examples. I may quote from the title page of this work, to give you an idea of the topics which it included. It contained the Elements of Algebra and Geometry, Geometrical Problems, Plane Trigonometry, the Mensuration of Heights and Distances and of Surfaces

and Solids, Conic Sections, Spherical Trigonometry, Elements of Navigation, Geographical, Geodetical, and Astronomical Problems; and there were applications, which deeply interested a rural and coastal population, to Specific Gravity, Tonnage of Ships, Weight of Cattle and Haystacks, Land Measuring, Artificers' Measuring, Gauging and Gunnery. Besides this truly catholic collection of subjects the book contained a pretty complete set of Logarithmic and Trigonometrical Tables, Tables of Square and Cube Roots, and of Compound Interest and Annuities.

I did not work problems in all these subjects, but a course was prescribed by the schoolmaster in Algebra, Trigonometry, and Mensuration of Heights and Distances and Solids, which was well illustrated by numerical problems, all of which had to be carefully worked out by the Tables and written neatly in MS. books, which were very thoroughly inspected. This gave what was in itself of value, a training in setting down, in a clear and orderly manner, numerical calculations made by means of logarithmic tables, an art in which students are often wofully deficient. The diagrams were in some cases drawings as well executed as boys could make them.

We did not perhaps learn to be rapid in algebraic manipulation, and expert in the art of reproducing proofs of algebraic theorems, and summation of series tricks; but we got from such a course some clear idea of the part which mathematics played in common and industrial life, and laid up a stock of ideas, and to some extent of principles, which was helpful in subsequent study. One subject of algebra was included which the later books tabooed, that of the elements of finite differences, or at least its application to series. This to me was a subject of intense interest, which I pursued later in the pages of De Morgan.

To an educationist of the present day this book of Davidson's may seem a mere *omnium gatherum* of mathematical scraps, and needless to say I do not suggest it was a model. But it was not *necessary* to study all the things which I have enumerated. I can answer for the interest which the collection excited in boys who used it. The author did not neglect any legitimate source of interest, geographical, local, or topographical. One problem has always dwelt in my memory, a problem which gave me my first notion of geographical triangulation, the very beginning of geodetic surveying.

A base line was laid down on the sea shore from a point B, at Burntisland School, to P, the end of the pier at the village of Pettycur (near the scene of the death of Alexander III.), a distance of 10,110 feet. From these stations a number of prominent objects are visible, Nelson's Monument on the Calton Hill of Edinburgh, the Island of Inchkeith with its lighthouse, the Martello Tower at Leith Harbour, North Berwick Law, and so on. The angles at the two base stations between the base and lines drawn to these objects, being measured, gave a number of triangles from which the distances between the stations and the objects, and between the objects themselves could be found. And to one who like myself had of an afternoon walked to the high ground at Dunearn above Burntisland, to look at the sublime panorama of shore and sea and island, and behind, like the stage of a gigantic but distant amphitheatre, the palaces and towers of Edinburgh dimly veiled by smoke with their guardians the Lion of Arthur's Seat and the Pentlands, the solution of such problems had an interest that no mere diagrams with their letters, A, B, C, etc., could ever afford. I suppose that in every Scot, if the *perfervidum ingenium* has not been extinguished in him by a training in the school which has for its motto *nil admirari*, and its rule of "good form" the suppression of all enthusiasm, there is a combination, which to some may be fit subject for a sneer, of regard for the prosaic and useful and feeling for romance which is inextinguishable. I would suggest, and this is my present point, that perhaps the problems set to our students might frequently be made more humanly interesting than they usually are.

An advanced pupil of an old Scottish Burgh School would not have been

puzzled by many simple problems which defied the efforts of some of our young men from schools of much greater pretensions and even from the universities, who were subaltern officers of Artillery and Engineers in the great war. I heard of an abortive attempt which they made, by some simple device for determining angles of altitude, to measure the height of an object, I fancy it was an observation balloon, which was vertically above a point within the enemy's lines. They got the angle of elevation, but to find the base of the right-angle triangle, which they naturally thought of, was beyond them. The simple expedient of getting two angles of elevation of the balloon from the extremities of a base wholly within their own lines had not occurred to them. Problems such as arose continually during the war could have been solved at once by some of those pupils of the old-fashioned teachers of practical mathematics. I do not say that there were not many competent young officers, but there certainly were many, in the scientific corps of the army, whose skill in elementary mathematics was not remarkable.

But time is passing, and I have said very little about my subject, that of dynamics. It was my fortune to spend some years in Edinburgh and to attend classes in the old Watt Institution and School of Arts which was founded by Leonard Horner and others. There we went through a certain curriculum consisting of Mathematics, Natural Philosophy, Chemistry and English, and graduated, so to speak, as Life Members of the Institution. Our claims as Members were forgotten when the Institution became the Heriot-Watt College, and I trust that this forgetfulness may yet meet with appropriate retribution. A little more sentiment and imagination would, I venture to think, do no harm to some of our friends who manage things in the Modern Athens.

However, I testify to my gratitude to the Old Institution, from it I obtained my first incentive to scientific work, and its Lecturer on Mathematics, James Pryde, a self-taught and sound mathematician, the compiler of various treatises for Messrs. Chambers, and the editor of successive editions of their mathematical tables, helped me in many ways. He invited me to his house, showed me to my great delight his mathematical library and his surveying instruments. He had some beautiful books, the works of the great French mathematicians, Hamilton's books on Quaternions, a branch of analysis which Pryde cultivated with enthusiasm. He encouraged me to read calculus, and curiously enough gave me a book on Fluxions, Dealtry's *Fluxions*, to begin with, which contained many good dynamical examples. I sometimes dip into old Dealtry yet, and though the book is an example of the "dotage" of the University of Cambridge in 1816, five years after the *Mécanique Analytique* of Lagrange had passed into a second edition, it is exceedingly interesting and instructive. This book opened up a field of work, the study of dynamics, which I have never altogether forsaken, though I have often felt greatly discouraged, and been tempted to seek solace in other fields, for which it appeared from time to time I might have more capacity. I did not find dynamics an easy subject, it is not easy; but at that time there were far more difficulties than now exist. Those who have read Tait's celebrated Lecture on Force to the British Association will understand what the difficulties are to which I refer. At that time the doctrine of energy was far from being so well stated and enforced with illustrations as it is now, though even now it is not the complete, polished, and rounded body of theory that some authorities and, as it seemed to me, Thomson and Tait supposed it to be. Many things besides its position in the theory of relativity remain yet to be settled.

The treatment of the foundations of dynamics by these authors, "T and T'" they were familiarly called, in the *Natural Philosophy* was suggestive, but in a certain degree unsatisfactory. Tait wrote a good deal from time to time on *Newton's Laws of Motion* and on *Force* and related conceptions; Thomson, except in the *Natural Philosophy*, wrote not at all on these subjects. They do say that "all motion, that we are or can be acquainted with, is

relative," but there they stop. No discussion of reference frames, of what is a straight line, of what is meant by uniform motion, encumbers their pages. Newton's *definitiones sive leges motus* are taken with emphatically expressed approval as the best foundation of the whole matter. Only on the side of energy is some addition said to be desirable, and the only really modern change alluded to is the equivalence of heat and work as established by the researches of Joule.

In his review of the *Natural Philosophy* Clerk-Maxwell called attention to the introduction of the method of Lagrange in an eloquent passage. "The two northern wizards were the first who without compunction or dread uttered in their mother tongue the true and proper names of those dynamical concepts which the magicians of old were wont to invoke only by the aid of muttered symbols and inarticulate equations." I have always understood that if the formula of a spell was not correct or complete, the invoker of spirits was in great danger. The spell of the northern wizards was not free from defect. The paper of Ferrers appeared some time before the *Natural Philosophy*, but there is no indication that the "northern wizards" were conscious of the existence of a large class of important cases of motion over which the spell of Lagrange has but limited power. More than one wizard has come to grief over these.

I wonder what the authors of the *Natural Philosophy* would say if they came back and read the numerous and voluminous discussions of relativity, and the disquisitions on Einstein's Theory by so many writers of all degrees of qualification for the task of dealing with such high themes, which we are favoured with in every scientific journal of the present day. I can imagine some of the comments.

It was my lot to be assistant to Thomson (not then yet Lord Kelvin) when the second edition of "T and T′" was passing through the press, and I wrote to his dictation, to a great extent on the proof sheets, most of the large additions which were made in Part I. Those in Part II. were made mainly by George Darwin. I have all the editions of the book, and it is interesting to compare them. In the German edition of Wertheim, Helmholtz, as is well known, deprecates Tait's somewhat not very coldly judicial treatment of the claims of Joule and Mayer. But take § 343 of the new English edition, which brings in the consideration of vibrational, or as Thomson called it, from the cycloidal pendulum, cycloidal motion. It became necessary to add a section 343*a* to give an exposition of the general theory, in generalised coordinates, of the motion of a system acted on by positional forces proportional to displacements, and motional forces proportional to speeds, and having kinetic energy expressed by a quadratic function of the speeds, with constant coefficients. This overflowed into 343*b*, and so on, until the flow of additions was exhausted in 343 p, in all 14 pages of small print.

These additions dealt with the vastly important subject of the significance of equality of roots of the determinantal equation, and of the physical significance of such roots, according to the circumstances in which they occur. It will be remembered that all cases of equality of roots were lumped together by Lagrange, and apparently also by Laplace, as indicating instability of motion, an error due to the neglect of consideration of particular cases.

Then came § 345, which in a similar manner swelled out from a single section 345 i. to § 345 xxviii. This part dealt with gyrostatic terms, and gave the theory of what Thomson called *gyrostatic domination*. In this connection I may remark that the cases of gyrostatic motion discussed in § 345 x. and illustrated with diagrams of apparatus which have been realised, might have been further explained with advantage. The description given is hard to understand: I am not sure that it is correct. I have myself tried to clear up this difficulty in what I have written on the subject.

I have been thus personal and reminiscent, because I think that I can in connection with these historical incidents and impressions indicate how

inspiration may come from the example and teaching of a great man, and how also inspiration may to some extent be limited and hampered by the prepossessions which often exist in a mind of even the first order.

It is inspiring to be in contact with an investigator engaged in making scientific discoveries, that is, as in this case, in rendering explicit ideas which already existed more or less unconsciously in his mind, and in following out the implications which these ideas involved. Many things crowd on my recollection which, lest I weary you, I will not here refer to; perhaps on some other occasion I may speak of them; if I don't, it will not I think greatly matter. Inspiration there was, but for Thomson's assistants that was not so great a thing as might appear. Everybody was engrossed in the work which was being carried on in Thomson's study and in his laboratory, and the individuality of the workers was apt to be overwhelmed and lost, certainly a regrettable result.

As I have hinted, Thomson was a man of somewhat strong preconceived opinions. A systematic teacher he was not. Immersed in the business of advising cable companies, occupied with nautical and other inventions, President of learned Societies, he was much from home, and one could never tell before the lecture began what aspect of his subject he would deal with. The task of his official assistant was a difficult one.

On two or three points of teaching practice, Thomson had made up his mind to regard what seems to me the worse as the better part. He was a consistent opponent of vector analysis, indeed, as he once phrased it, of that whole "vapid affair of vectors." Tait was a vectorist, of the school of Hamilton, and a consistent and vigorous advocate of the Hamiltonian system in all its details. There are difficult passages in his treatise on quaternions in which he declares that a certain mode of dealing with the differentiation of a quaternion is absolutely imperative, a subject on which Heaviside has written with eloquence of a bitter but forcible sort. Nevertheless there is no reference to vectors or vector analysis in "T and T'," not even in the introduction to the geometry of motion with which that treatise commences. I remember that once, when it was suggested by someone that the hodograph might be introduced in the elementary lecture course, his saying that, no doubt the thing was very pretty, but that it was unsuited for discussion with students. His method of proving that the acceleration of a particle moving with uniform speed v in a circle was towards the centre and of amount v^2/r, was the old-fashioned one of taking a small arc described in time 2τ, and showing that the versed sine $r(1-\cos\theta)$ of the arc was $\frac{1}{2}a\tau^2$, which of course gave a (the acceleration) as v^2/r.

Another incident, which occurred in 1898, illustrates this attitude of mind. In something I had then written (I had fourteen years before gone to teach physics in North Wales) I ventured to say that I had endeavoured to emphasise vectorial ideas, but had not so far used the technical calculus of vectors. I received from Lord Kelvin an epistle couched in very vigorous language, in which he expressed the hope that it might be long ere I made use of vector analysis. Enclosed was a copy of a letter which he had written to Mr. R. B. Hayward, who had sent him a little book on double algebra (or some such title). That letter he said was written very politely, "as to a stranger," and I certainly wondered what he would have said to one with whom he was intimately acquainted, and who had perpetrated a similar book. Of that I had some indication in what he went on to say to myself. He referred in terms of very great admiration to Hamilton's dynamical papers, which, as he said, were written in the heyday of Hamilton's power and enthusiasm. Certainly nothing Lord Kelvin could say could exaggerate the importance of Hamilton's contributions to general dynamics. It was the initial contribution (I shall not say the starting point) of a long series of dynamical investigations which were continued by Jacobi, Gylden, G. W. Hill, Henri Poincaré, and many others, and which have resulted in numerous improve-

ments and discoveries in modern analysis. I need not particularise, for much of this work is described in Whittaker's *Analytical Mechanics* (I wish he had said *Analytical Dynamics*), where also our President has brought it into focus with Contact Transformation Theory.

Now, with the utmost respect to the memory of my great master and teacher, I cannot assent to the view that Hamilton's insistence on the vectorial quality of dynamical things is any sign of decadence of power. See how beautiful is his theory of the hodograph. The hodograph was invented, years before Hamilton hit upon it, by Möbius, and will be found expounded in the *Mechanik des Himmels*. Möbius was undoubtedly a man of great originality, but he made no use whatever of the hodograph idea. In the hands of Hamilton it was a key for the unlocking of all kinds of dynamical secrets; and in modern dynamics is in no way confined to particle motion, but is of great importance in the motion of a rigid body.

I was taken to task by the late Mr. Philip Jourdain for insisting on the importance of vector ideas, while abstaining (in my book on *Gyrostatics*) from the habitual use of vector analysis. I am still unrepentant, and I propose in what remains to me of the hour allotted to me to endeavour to give some reason for the faith which I possess and avow. In Thomson and Tait, and other treatises even of the present day, it is usual to speak of a *velocity* of so many feet per second—the velocity of a train for example—when there is no intention of referring to the element of direction. There seems to me great impropriety in using the word *velocity* for what is merely a *speed*. Speed is a good Anglo-Saxon word, and is constantly used by the people in the scientific sense of the numerical rate of displacement. *Velocity* is a Latin word, comparatively little used by the people, and well adapted to denote the complete directed quantity. I have no fad about the use of Anglo-Saxon words; I wish by the way that the writing of "forewords" by patronising publicists and politicians could be stopped.

Let me take an illustration of my contention that while in a large range of cases the idea of direction is unimportant, and *speed* is the appropriate word, there are cases in which to keep the idea of direction in the forefront is imperative and to forget it is fatal. An aeroplane is describing a spiral, making successive circuits in a period of perhaps two or three minutes or less. The speed is so and so in feet per second, and may be practically constant. The *velocity* however is constantly changing. At one end of a diameter of a spire of the path the velocity is different, literally *toto coelo*, from what it was at the other end. The loose language used makes it the same velocity, and looseness of language (I do not mean the use of colloquial phrases) always begets looseness of thinking.

The want of appreciation of the vectorial principles of rotational motion has been to me, during the last four or five years, a great marvel. I have listened to eulogies of a little top which was supposed to indicate the vertical on an aeroplane. It was declared to behave splendidly because when set up on a table and deflected, say 20°, it came back to the vertical in a few seconds. It did not seem to occur to these applauding gentlemen that if the thing returned quickly under a small couple, which was what it did, an equally small disturbing couple would deflect it quickly from the vertical. That was also exactly what happened. The toy could stabilise no sight or other appliance, even if it could be trusted to act as a gyroscopic level.

I have said elsewhere, and I repeat it now, that "the discussion of dynamical and especially of gyrostatic problems in the lecture-room and in text-books has either been restricted to one or two simple special cases, or, when more comprehensive, has been far too much an affair of Euler's or Lagrange's equations, which, by providing a walled-in path along which the mind can travel, will draw its attention from the incidents, often exceedingly instructive, which attend its progress." The gravamen of my complaint is that the discussion is too purely mathematical. It ought to be mathematical, but it

should also be physical. The phenomena are physical, and ought to be studied in the lecture-room and workshop of a properly equipped institute of natural philosophy, where real dynamics is made an important subject. The procedure which prevails at many of the universities and colleges—and at some other places—may be good for obtaining marks in examinations of a certain kind, but as a training for dealing at once and from first principles with the practical scientific questions which arose during the war, I say that it is sadly deficient.

As a result of 35 years' experience of the teaching of dynamics, I have come to the conclusion that a reform of the elementary teaching of the subject is much needed. I have seen all the papers on elementary dynamics set during the last twenty years, for the Joint Board (now superseded) which conducted the Entrance Examinations of the Scottish Universities, and a good many of the papers of similar standard set in other Universities. These papers have as a rule not been such as to afford much help, to say nothing of inspiration, to the teachers of dynamics in our schools. Dynamics is the foundation of *Physics* and is an excellent subject if it is well taught, that is, if the physical ideas involved are well brought out and fully illustrated by a constant appeal to common experience and observation, and if the instruction is logical and clear. And the utmost care should be taken, very much more care than is usually exercised is required, in examining in dynamics, because the entrance examination papers of the universities should give a lead to the teachers in the schools. The wording of the questions should be logical and the questions themselves should be chosen so that cram may be as far as possible discouraged and discredited. Such a question as "State Newton's second law of motion" is an example of a form to be sedulously avoided. And the examiners should, under penalty of some kind, be required to work out their own questions, and send solutions with their draft papers, so that they may be fully conscious what they propose.

What usually happens seems to be something of this sort. The examiner receives a letter saying that a paper is required by such and such a date, more or less distant, and he acts according to somebody's recipe for dealing with bills, "file 'em and wait a bit," with the omission of the filing. Then comes a more urgent letter, and he jots down hastily a few questions, perhaps over his coffee at the breakfast table. None of the questions is so framed as to be really searching, some of the problems proposed are ridiculously easy, others are suggested that could hardly be answered in the allotted time, and without books, by the whole committee of examiners, if they put their heads together. More than once I have seen the following question, or something like it, proposed, which illustrates what I have said about examiners working out their own questions. "A particle describes a vertical circle 2 feet in radius in a period of half a second: find the speeds of the particle at the highest and lowest points of the circle." The examiner thought he was proposing something delightfully easy for the schoolboy examinees, for whom he no doubt felt a glow of generous sympathy. Alas the question cannot be solved without finding the modulus k of an elliptic integral K of the first kind, such that $kK=1$.

I do not want to remove the teaching of dynamics from the mathematical classroom to that of experimental physics. It has been suggested that I do, but that is a mistake. But I say that where there is a department of natural philosophy the teaching of dynamics should be carried out in that department. North of the Tweed we have the study and teaching of both mathematical and experimental physics carried out under one roof, under one direction, by professors and lecturers in daily and hourly association with one another.

This most desirable arrangement arose in this way. Two hundred and more years ago the staple subject of instruction at Edinburgh, for example, in natural philosophy was dynamics. It was in fact what used to be referred to as the "Newtonian Philosophy," which was first taught in any university, so

far as I know, by David Gregory, one of the celebrated predecessors of Professor Whittaker. I may remark that the Gregorys were not a mere family, they were like the Bernoullis, a mathematical dynasty: David Gregory of Kinardie, brother of James Gregory of Aberdeen, also a great physicist and mathematician (Gregory of the telescope), had three sons who were all Professors of Mathematics, David first at Edinburgh then at Oxford, James at Edinburgh, in succession to his brother David, when the latter became Savilian Professor, and Charles at St. Andrews.

To this foundation of Mathematical Physics has been added in Scotland Experimental Physics, and collections of apparatus and lecturers, so that the study of physical properties of bodies is carried on as I have said under one roof and by teachers and workers in daily and hourly association. Thus physical ideas are less likely to be neglected, and the study of dynamics runs less risk of becoming an affair of pure mathematical exercitations, than if it were committed to the care of a merely, or shall I say purely, mathematical department. It is to a Natural Philosophy Institute such as I have indicated that I wish the teaching and examining in dynamics entrusted.

This is my idea, and I am glad that this arrangement for teaching physical science exists in Scotland. I have deeply regretted to find a tendency to bring in the evil Southron example, in some proposals that have been made in connection with the revision of the regulations for our Degree of B.Sc. in Pure Science. There it has been proposed to set up the extraordinary cross division of Natural Philosophy and Experimental Physics. The philosophical, that is, in the main, the mathematical element, must on no account be absent from experimental science (indeed I seem to recall the phrase "experimental philosophy") and equally a reference to experiment must be a vital part of mathematical physics. Of course the emphasis in different cases may and should be placed on one or other of the two sides or divisions of natural philosophy. One without the other is of little value. Natural Philosophy comprises all, and real sound teaching and progress can only be made by maintaining the "auld alliance." The nearly complete divorce of experimental from mathematical physics, the too frequent absence in teaching of the combination we have in natural philosophy is I know deplored by some of the most thoughtful men at Cambridge and elsewhere.

Well then, the teaching of dynamics carried on in an Institute of Natural Philosophy should, even in its elements, be vectorial in spirit. If a teacher cannot infuse this spirit into his work without constantly revelling in vector analysis, he is a vectorist, pure and simple, and is much better elsewhere.

But it is possible, and I say again that it is extremely desirable, to insist on every possible occasion on the directional quality of the quantities dealt with. It has been urged that the most elementary students must be treated in a specially tender manner, and have their attention directed in the first place to the case in which the rate of change of the quantity considered has the same direction as the quantity itself. Now here I join issue with the teachers who hold this view. By all means direct attention to this case, but I am not at all certain that it is desirable to begin with it. The normal case in nature is not this at all, and nature certainly has no compunction about plunging students at an exceedingly tender age into all the variegated dynamical action of a work-a-day physical world. If the student is old enough to begin the serious study of dynamics, and he surely is when he enters college, he is able to face the reasoning required for the general case. If he does not begin with it he is in great danger of forming an erroneous idea, which clings to him afterwards, that acceleration is essentially along the line of motion, and the acceleration in curvilinear motion may remain for him an inscrutable mystery.

Velocity and acceleration are the first thing which the student has to obtain correct notions about. The teacher discusses composition of velocities, he must do this even with beginners. In doing so he insists, in his specification, on the element of direction, and shows that if he represents two velocities

by the lines OA, AB, drawn from a point O, in the proper directions, indicated by the sequence of letters in each case, their resultant (in a properly explained sense) is completely represented by the single velocity OB. He makes clear of course what this means. It is then an easy step to go further and say, "if at the beginning of an interval of time the velocity of a particle is OA, and at the end is OB, the change that has taken place in the interval is to be estimated by considering what is the velocity, which, *existing along with the initial velocity*, will give by the process of composition the final velocity OB. This obviously is the velocity AB, which clearly is the only thing entitled to be called the change which has taken place." The manner of growth during the interval is another thing, but its treatment is the same: the interval of time may be broken up into parts, and the change for each part found in the same way.

The average acceleration for the interval is simply the whole change AB, taken per unit of the time-interval, and has therefore the direction of the change AB. From the change for each part of the time-interval comes in like manner the average acceleration for that part, and finally the acceleration at a particular instant.

Here I may say that I have found it illuminating, or at least helpful, to spend a little trouble in the consideration of various points, which are, I think, rather frequently slurred over. The idea of an instant has been examined at some length by various writers, and by some dismissed as involving contradiction. The iron bob of a pendulum is a complex of electric charges in motion, so that it is said, the ball is not the affair of an instant. But it is a certain average of impressions, which so far as our power of examination goes remains invariable, and we think of it as continually existing unchanged. Hence, if we have the idea of duration we must have the notion of successive intervals of time, and an instant is the point of time, so to speak, at which two successive intervals butt up against one another, and at that instant we regard the object of our average impressions as existing. It is the final termination of the prior interval, the beginning termination of the subsequent interval. But it is not an interval of time at all. In the same way a line is the common boundary of two surfaces, but is not a part of either. This conception enables us to speak of the bob of a pendulum as being, when it is at a turning point in its motion, at rest. It is at rest at the instant, but it does not remain at rest for any interval of time however short.

Again, to take the old problem of the Eleatics, a particle moving along any path occupies all the successive positions possible. It is in one position at a certain instant, and is moving then with a certain velocity, but this position (and it may be also this velocity) is not characteristic of the moving particle *during any interval of time*.

We may extend this discussion to the growth of vectors of a more complicated kind, and the whole subject gains enormously in clearness when this is done. The growth of vectors is clearly and elegantly treated by vector analysis, but, even without that calculus, it is possible to obtain much help from vector ideas. The following simple process is sufficient for the most difficult problems of moving axes.

Think of any line Oq, drawn from a fixed origin O, and fixed in space. Associated with that line is a directed physical quantity, a momentum, say, along it, or an angular momentum about it, but whatever the quantity may be it is represented graphically by a measured line drawn along Oq. Let us suppose that the positive direction is from O towards q, so that this is the positive direction of the associated vector, which we shall denote by M. The value of M changes with the time at rate $\dot{M}$, which is here, by the way, associated also with the direction Oq. Let Op be another line, at right angles to Oq, and let us suppose that this line is moving with the body. Associated with Op is a vector L, of the same kind as M let us say, and further let Op be

moving in the plane pOq with angular speed ω towards the fixed direction Oq. Then there is a rate of vector growth for Op, of amount $L\omega$. The whole rate of vector growth for Oq is therefore $\dot{M} + L\omega$. We take this as a fundamental result, and give one example of its use in rigid dynamics, namely, to the proof and interpretation of Euler's equations for the motion of a rigid body about a fixed point.

But first take a still simpler example. A particle moves with speed v in a curve, say a circle of centre C and radius r. Its momentum is mv, and it is in the position P say. The momentum is a vector in the forward direction along the tangent at P. But this vector is turning with angular speed v/r towards the radius PC. Thus momentum from P towards C is growing up at rate $mv \cdot v/r$, that is, mv^2r. If the curve is not a circle r is the radius of curvature, and there is moreover the acceleration $m\dot{v}$ in the line of motion.

Take now the more complicated rigid body example. Set the rigid body be turning about a fixed point O. Take as axes the principal axes of moment of inertia OA, OB, OC for the point O, and let the body at the instant be turning about these axes with the angular speeds p, q, r. Hence the components of angular momentum about the axes are Ap, Bq, Cr if A, B, C be the moments of inertia. These quantities are associated with the axes in their instantaneous positions, and for these instantaneous positions there are the three time-rates of change $A\dot{p}$, $B\dot{q}$, $C\dot{r}$. The axes being fixed in the body these are time-rates of variation of A.M. (due to the changes proceeding in p, q, r) for fixed axes with which the moving axes at the instant coincide.

Now, besides these there are rates of growth of angular momentum due to the fact that the position of the body is changing. OC is moving towards the instantaneous position of OA with angular speed of about OB, carrying with it its angular momentum Cr. Hence, by the fundamental result given above, there is a rate of growth of angular momentum about the position of the axis OA of amount Crq. Again, the axis OB is moving *away from* the instantaneous position of OA with angular speed r about OC, carrying with it the angular momentum Bq. Hence there is a rate of growth of angular momentum $-Bqr$ about the instantaneous position of OA. The total rate of growth of angular momentum *about the fixed axis, with which OA coincides at the instant, is*

$$A\dot{p} - (B - C)qr,$$

and this is equal to the applied couple about the same axis. Thus we get Euler's equations of motion.

In some cases we may consider the vector representing the resultant angular momentum, and the total rate of change due to the resultant couple. The latter is the velocity of the extremity of the former vector. If a sequence of such vectors all drawn from one origin O be supposed erected in space, the curve in space on which lie their extremities is the hodograph for the rigid body motion, and the rate of production of angular momentum at any instant is measured by the velocity of an imaginary particle or point, which moves so that it is at each instant at the extremity of the vector which for that instant represents the angular momentum of the body. The displacement and motion of this representative particle represents the motion of the body and its progression of changes.

I have been informed that "all this is in Routh." I have not found it there; though of course it can be put there by reading into Routh's discussions a sufficient number of things. Routh and other purely Cartesian writers speak of $(B - C)qr$, and the two other similar quantities as "centrifugal couples," a perfectly artificial, unphysical, and almost unintelligible mode of regarding the matter.

There are two kinds of accelerative terms, one depending on the flux of time, the other on the motion of the axes of reference. These correspond to the

two terms on the left of the typical Lagrangian equation in generalised co-ordinates

$$\frac{d}{dt}\frac{\partial T}{\partial \dot{p}} - \frac{\partial T}{\partial p} = P,$$

where T is the kinetic energy expressed as a quadratic function of the speeds $(\dot{p}_1, \dot{p}_2 \ldots)$ with coefficients which are functions of the coordinates $p_1, p_2, \ldots$. Equations of this type exist only for what have been called *holonomic*, or *holonomous* systems, that is systems the kinematic conditions of which are completely expressible by finite equations. By a proper vectorisation of the quantities and application of the simple methods suggested above, and based on vector ideas, we may deal with the most general cases without technical vector analysis. [For other examples the reader may refer to a *Treatise on Dynamics*, by Dr. J. G. Gray and the author of this paper.]

By continually referring to first principles and applying them the solution of problems ceases to be a dull mechanical process, the true dynamical spirit continually animates the worker, and dynamics becomes a living science, illustrated as it should be to the learner by the commonest events of life and ready to encounter new and unexpected problems whenever these arise in our experience. A. G.

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51. This process seems to be peculiar to mathematics: to go on using a word or a sign, without any knowledge of it, except that it is a word or a sign, to be used in a certain way, until the results of that use point out the meaning which the word or sign ought to have had, is a strange idea when presented for the first time. But, nevertheless, it has been used out of mathematics: in logic, for example. Wallis, the first mathematician, I believe, who formally introduced interpretation into algebra, had previously made use of it in logic. In a disputation (at Emmanuel College, Cambridge, in 1631), whether a singular proposition is to be held universal or particular, his thesis (printed at the end of his logic) decides the question by interpretation, as follows:

A singular proposition, such as "Virgil was a Roman," is to be taken so that the rules of logic may be applied to it. From the premises "Virgil was a Roman," and "Homer was *not* a Roman," it certainly follows that "Virgil was not Homer." Now, if the two premises be particular propositions, there can be no conclusion: from "some *A*'s are *B*'s" and "some *C*'s are not *B*'s" nothing can be inferred. Consequently the premises must be considered as universal propositions.—De Morgan, *Diff. and Integral Calculus*, n.p., 768.

52. Or wilt thou shiver in the realm of Frost,
Ten thousand years fast fetter'd to the Pole?
Or, to the centre of the deep earth toss'd,
There tumble, free from Gravity's control,
In many an antic gambol.—Moultrie's *Maimoune*, xxiii.

53. There's nothing in the world (that is in Trinity)
To make us poets happy;—I detest
Your Hebrew, Greek, and heathenish Latinity,
And Mathematics are a bore at best.—Moultrie, *Sir Launfal*, xii.

54. The student must here avoid the mistake which . . . I have twice fallen into in the course of this work.—De Morgan, *Diff. and Integral Calculus*, n.p., 372.

55. Mr. Sharp . . . not only an excellent mathematician but a good mechanick . . . was present when Facio, an ingenious man and the inventor of a new contrivance of applying precious stones to watches, presented their watches to have the approbation of the Royal Society, with explanations. Prices of Hawkesbee Airpumps, £5 to £35. Those of £25 are noble instruments indeed. Patrick's portable barometers are two guineas the lowest. —Derham to Richard Towneley, Mar. 23, 1705 (Ellis, pp. 315).

MATHEMATICS AND COMMERCE.

By A. N. Shimmin, M.A., Lecturer in Economics, University of Leeds.

My first duty in approaching this subject is to make it clear that I am not a mathematician seeking to demonstrate the application of mathematical methods in the field of economics. My aim is to indicate the phases of the general economic problem which demand the co-operation of mathematician and economist as the only means of arriving at sound judgments and giving the scientific study of industry and commerce a value which will command increasing respect and attention.

London has been described as "a hive for the millions of human bees that pass in and out of its doors, swarming or working, idling or pursuing in silent and unconscious obedience to a law which not one of them will ever live to understand." Vividly that description puts before us the subject-matter of economic study, and drives home the necessity for closer analysis of these processes of 'swarming,' 'working,' 'idling' and 'pursuing' which go to make our economic system. Essentially we are dealing with human beings. There is increasing stress on the human side of industry, and a definite quickening of the demand that the 'laws' under which we live shall be discovered and made known to all. Thus the workman objects to being regarded as a 'hand,' a 'tool' or a 'cog in the machine,'—but his remedy is too often an ill-balanced philosophy of industrial reorganisation, not a dispassionate study of existing facts and possibilities. Equally, the capitalist and enterpriser plead for the protection of their interests, for the 'stability of industry,' and often fail to discern the broader tendencies at work. Arguments which will effectively adjust this difference of views cannot at the moment be supplied, because our survey of the facts involved has been too cursory or spasmodic to afford a closely reasoned account of the interplay of economic actions.

A valuable beginning might be made by recognising the influence of the change from private to large-scale enterprise in industry. Under the earlier régime industry or commerce was the affair of the private-firm, which in all essentials was an industrial family, with a continuous history and an economy more closely akin to the village than the city. Members grew up with the business, and the enterprise was such as they could furnish from among themselves. The adminstrative problem existed mainly in the sense in which it presents itself to a modern household. Risks were minimised by the smallness of scale of operation, and efficiency was directly dependent upon personal oversight and co-operation. A careful study of any one unit would have revealed the characteristics of the system as a whole.

Large-scale enterprise—the most marked economic development of the last fifty years—has brought highly important changes. The key to the situation is specialisation, but we know far too little about it as yet to be able to give true meaning to the complex groups, activities and standards which are its outcome. To the mathematician the economist can present a wide field for research in connection with the economic life and conduct of those who staff our modern businesses. Specialisation means groups, with perpetual change in the content and relationship of those groups. Successful administration is the clue to successful specialism, but everyone will admit that it involves a knowledge of conditions which is rare in industry to-day. If, for example, we were in the habit of recording more accurately the distribution of the masses of material and labour power upon which we depend, we might go a long way towards eliminating those 'maladjustments' which bring unemployment, bad trade and serious social distress. In the absence of data which would give precision to economic effort we are compelled to rely on guess-work, rumour and the crude adjustments of competitive bargaining.

In industry proper the economic problem may be summed up in the words 'management' and 'efficiency.' Management is essentially administration nowadays, and all the units from the one-man business to the trust may be regarded as parts of a huge scheme of production, demanding of the organisers of industry large administrative capacity and wide and accurate knowledge of the factors upon whose grouping the success of the scheme depends. It is no longer satisfactory to identify good management with a reasonable margin of profit on turnover. Mere subdivision of tasks may easily involve waste and overlapping—hence the call for greater efficiency. And just at this point there emerges the most pressing need for accurate investigation of conditions and the formulation of more productive plans of working. Tradition dictates that we do the best with the things we have to hand, so long as industry can be made to pay. No wonder our factories, workshops and towns present such overwhelming evidence of bad management, inefficiency and stunted growth. The scientific treatment of industry demands that we rest content with nothing less than the best,—but the description of what is best is quite another matter. Are we to accept the lead of the 'scientific manager' and concentrate on the elimination of waste effort, or must we follow the accountant who urges that costing is the most promising method of applying scientific calculation in industry? Both scientific management and costing are in their infancy, but they are welcome indications of the belief that the problem of production will bear a much more detailed analysis than it has received during the rule of *laissez-faire*.

The commercial problem is no less complicated. The spread of world specialism has created a network of markets, and intricacies of banking, finance, exchanges and prices upon which only the expert may venture with any hope of success, and the experts are too few in relation to the size and importance of the transactions undertaken. The recurrent charge of theorising ought not to deter us, because our material springs right out of the heart of the business world. We need firmly to stand by the contention that a more detached view of things should be cultivated, and that the significance of data should be learned by comparative study.

It is obvious, then, that the study of economic problems in its present form contains many gaps, and proceeds on generalisations which frequently have to be drawn from slender evidence. On the other hand, the popular presentations of the subject are crowded with fallacies and misleading views which are not easy to refute. The surmounting of both difficulties may be achieved if the mathematician will co-operate with the economist in the interests of stricter study.

In the first place the study of mathematics demands *accuracy*, and the student is bound to definition of terms. This quality is frequently the envy of the student of economics, and one need only refer to the varying content of such terms as 'unemployment,' 'sweating,' 'minimum standard of living' to detect the uncertainty of many of our calculations and the origin of much bitter and uninformed controversy.

Secondly, the mathematician is able to train a student to a proper appreciation of *causal relationships*—so highly important in the world of industry and commerce. We pride ourselves on working explanations of the influence of prices on demand, the connection between hours of work and output, the bearing of wages on standards of living or of transport charges on the distribution of traffic—but I do not think the boldest economist would win through a mathematician's inquisition in those matters. That is not a disgrace, it is a weakness inherent in the rapid growth of the science. At the same time it is the mathematician's opportunity.

A third point is the value of training in the use of estimates. Our ideas of national wealth, the distribution of income, the yields of charges and taxes, of trading returns and occupations are based on estimates, and, in view of the different results presented from time to time, it is highly desirable to

equip the student of commerce with a means of checking the methods by which data are treated. I would like to refer at this point to the harmful use of percentage statements. Frequently wrong impressions are given by the use of percentages, because it is so fatally easy to omit desirable discriminations in the use of units, data, or terms.

This suggests the fourth point I wish to stress in the general connexion between mathematical training and economic study—the sense of proportion. The more closely one enquires into the nature of economic workings the more it becomes apparent that proportion is the secret of success in specialised working. In the case of demand we have the play of preferences for various commodities or services. Theoretically we seek to secure 'equimarginal satisfaction' in our expenditures, but who can defend the 'inverted standards' of living which give undue prominence to the consumption of, say, tobacco and drink? In production the need of proportion is everywhere apparent. Sound business management pays close attention to arrangement and relative costs of factors. Nationally we are only just approaching the problem of proportioning our effort to the urgency of our needs. Public sentiment favours the building of houses rather than picture theatres, and a serious suggestion has been made that a difficult financial situation should be met by the rationing of credit in the interests of the most 'productive' undertakings. And when we discuss the division of the product of industry, the charges which industry will bear, and the famous 'vicious circle' of prices and wages, we realise that a sense of proportion is a great asset in the analysis. How damaging the mistaking of a part for the whole may be we can realise by taking the case of prices. It is a very common error to quote price variations for all commodities in terms of the changes affecting one article or one group. A grounding in the construction and use of index numbers can prevent this. Wages arguments, again, are vitiated by incomplete surveys, and we hear a good deal more about munition workers' fur coats and grand pianos than we do about the lot of the tramway worker or the agricultural labourer. To take only one other instance, how many people have been misled by the inflation of our currency? "There's plenty of money about," and so, presumably, we are a much wealthier nation. I repeat, it is the proportion in which things exist and are used that matters most.

The special mathematical problem emerges with the development of large groups, classes, and series in industry and commerce. What may strike you as the drudgery of collecting and tabulating data has still to be undertaken as the preliminary step in the treatment of many difficult problems. But for the economist this description and analysis of groups and aggregates is vital, "for when investigation is extended over a large area, regularity is obtained, conformity to general laws is visible and new methods of description are required, while observation of a few cases suggests only chance and chaos."* The scope for enquiry of an extended type is great. Take, for example, the growth and transformation of industries. We know that there is some 'law' at work in the recruiting and displacement of labour,† but we need detailed accounts of its operations. The mobility of labour, the migrations of population, the distribution of internal trading, the problems of efficiency, payment, and output all afford rich ground for research and minute enquiry. In the study of fatigue in industry we have one of the happiest illustrations of the use of mathematics, economics, and psychology in combination.

The human mind is incapable of grasping a multiplicity of individual cases, and any preliminary training in mathematics should lead to statistics as the best means of handling groups of data. Counting is important, but estimating is equally necessary—for example, in all matters affecting population. By

* Bowley, *Manual of Statistics*, p. 2.

† See Chapman and Shimmin, "Industrial Recruiting and the Displacement of Labour" (*Proceedings of Manchester Statistical Society*, March, 1914).

analysis the statistician can reveal the meaning of data, and by calculation table or diagram he can reduce evidence from very disparate sources to a comprehensible form. The value of comparative study I have stressed already, and statistical work in the field of commerce is bound to involve a large amount of comparison. It is, in fact, the only means of discerning those tendencies which are expressed in economic laws, and it gives the best indication of what is likely to happen, and so contributes to the framing of industrial and commercial policies. In turn comparative study develops exact study of relationships,—and there is ample room for an accurate study of cause and effect in both home and international trading.

Our statistical work may not possess the accuracy of accounting in every case, but first-rate instances of the possibility of applying statistics to commerce are to hand in the computation of risks in insurance, with the development of actuarial science, and in the administration of reserves in the banking system. The mathematical Law of Probability suggests the possibility of a new commercial policy, which shall be able to eliminate many of our present risks and uncertainties, and it is significant that the systematic study of Imperial statistics has been undertaken. I recognise that the collection of material and the taking of satisfactory samples entail a vast amount of work, and large staffs of skilled investigators. But the perpetual loss resulting from error and miscalculation is surely sufficient stimulus for the work of training for a commercial career students whose outlook will be wider and whose methods of measurement will be more accurate than those commonly in use to-day.

The revised courses for the Commerce degree at this University are built on the principle that Commercial Law, Economics and Mathematics are the essentials of sound training. On the mathematical side Pure Mathematics must come first to afford mental discipline. In the second year Statistics, to be supplemented in the third year by Accountancy (including costing and business finance). A special bias to the training is afforded in the second and third year courses by means of options which will enable the student to specialise in the economics of the industry he proposes to enter, *e.g.* textiles, engineering, mining, or commercial administration at home or abroad. The concrete proposals for the first year's general mathematical training are embodied in the following syllabus:

1. Practical use of logarithms.
2. Arithmetical progressions, with practical applications.
3. Geometrical progressions, with practical applications.
4. Permutations and combinations leading up to probability.
5. Elementary theory of probability.
6. Plotting points leading to idea of gradient as representing rate at which one quantity varies with respect to another.
7. Differentiation of powers of x with practical applications to maxima and minima, tangents to curves, rates and velocities.
8. Integration of powers of x, with application to areas and volumes.
9. Application of Infinitesimal Calculus to errors and approximations.
10. Differentiation of logarithms with applications. Introduction of e as the logarithmic base leading to differential of logarithm being $\frac{1}{x}$.
11. Proof that the differential of e^x is e^x, and properties of e^x.
12. More advanced theory of probability. Use of probability curve $y = e^{kx}$.
13. Easy interpolation. Theory, with applications.

(Much of this course is to be found in the *First Course in Calculus* by Milne and Westcott. (G. Bell & Sons.).)

1st June, 1920. A. N. S.

LIFE OF JAMES STIRLING, THE VENETIAN.

By CHARLES TWEEDIE, M.A., B.Sc., Carnegie Fellow.

JAMES STIRLING, who takes rank with Maclaurin and Napier among the great Scottish mathematicians, belonged to the Garden branch of the family of Stirling. The Stirling family is one of the oldest of the landed families of Scotland. They appear as proprietors of land in the twelfth century.

In 1180, during the reign of William the Lion, they acquired the estate of Cawder (Cader or Calder) in Lanarkshire, and it has been in the possession of the family ever since. Among the 64 different ways of spelling the word Stirling, as recorded in the *Family History*, a common one in these early days of the family was a variation of Striveling. In 1448 the estate of Keir in Perthshire came into the possession of a Stirling. In 1534 or 1535 the Cawder and Keir branches of the family were united by the marriage of James Striveling of Keir with Janet Striveling, heiress of Cawder. Since then the main family has been, and remains, the Stirlings of Keir and Cawder. By his second wife, Jean Chisholm, James had a family, and his second daughter Elizabeth married (1571-2) the famous inventor of Logarithms, John Napier, Baron of Merchiston (near Edinburgh), whose estates in the Menteith marched with those of the Barony of Keir. This was not the first connection by marriage between the two families, for in the old Napier residence of Wright's Houses in Edinburgh (now Gillespie's Hospital) there is preserved a stone, the armorial bearings on which record a marriage of a Napier to a Stirling (I.S.) in 1399.

In 1613, Garden * in the parish of Kippen (Stirlingshire) first became a separate estate, being the gift of Sir Archibald Stirling of Keir to his son (Sir) John Stirling. The son of the latter, Sir Archibald Stirling, was a conspicuous Royalist in the Civil War, and was heavily fined by Cromwell; but his fortunes improved at the Restoration, and he ascended the Scottish bench with the title of Lord Garden. He succeeded to the estate of Keir, and a younger son Archibald (1651-1715) was given the Garden estate in 1668. Archibald's eventful career was one long chapter of misfortunes. Like the rest of the Stirlings he was a staunch Jacobite. In 1708, he took part in the rising known as the Gathering of the Brig of Turk. He was carried a prisoner to London, and then brought to Edinburgh, where he was tried for high treason but acquitted. He died in 1715, just about the outbreak of the Rebellion. By his first wife he had a son Archibald, who succeeded him. By his second marriage he had four sons and five daughters.

The subject of this sketch, James Stirling, the Mathematician, was the second surviving son of the second marriage. The sons were James, who died in infancy; John, who acquired the Garden estate from Archibald, 1718-9; James the Mathematician; and Charles.

The Armorial Bearings of the Garden branch of the Stirlings are:

Shield: *Argent* on a Bend *azure*, three Buckles *or*: in chief, a crescent, *gules*.

Crest: A Moor's Head in profile.

Motto: Gang Forward.†

YOUTH OF STIRLING; OXFORD.

Save for the account given by Ramsay of Stirling in *Scotland and Scotsmen in the Eighteenth Century* (not always trustworthy) little is known of the early years of Stirling prior to his journey to Oxford. Ramsay, it is true, says that Stirling studied for a time at Glasgow University. This would have been quite in accordance with Stirling tradition, for those of the family who became students had invariably begun their career at Glasgow University; and the fact that Stirling was a Snell Exhibitioner at Oxford lends some

* Pronounced Gardenne.

† Scotice for *Allez en avant*.
It reminds one forcibly of D'Alembert's famous advice to a timid mathematician: "Allez en avant. La foi vous viendra."

colour to the statement. But there is no trace of his name in the University records.

Addison in his book on the *Snell Exhibitioners* states that "Stirling is said to have studied at the University of Glasgow, but his name does not appear in the Matriculation Album."

But from the time that he proceeds on his journey to Oxford his career can be more definitely followed though the accounts hitherto given of him require correction in several details.

Several of his letters, written at this period, have fortunately been preserved. This fact alone sufficiently indicates the esteem in which he was held by his family, and their expectation of a promising future for the lad. Letters to his parents indicate his experience on the journey to London, and his endeavour to keep down expenses. "I spent as little money on the road as I could. I could spend no less, seeing I went with such company, for they lived on the best meat and drink the road could afford. None of them came so near the price of their horses as I did, altho' they kept them 14 days here and payed every night 16 pence for the piece of them."

He reached Oxford towards the close of the year 1710. He was nominated Snell Exhibitioner on Dec. 7, 1710, and matriculated on Jan. 18, 1710-11, paying £7 caution money. On the recommendation of the Earl of Mar he was nominated Warner Exhibitioner and entered Balliol College on Nov. 27, 1711. In a letter of the same year to his father (Feb. 20, 1711) he gives some idea of his life at Oxford. "Every Thing is very dear here. My shirts coast me 14 shillings Sterling a piece, and they are so course I can hardly wear them, and I had as fit hands for buying them as I could. . . . We have a very pleasant life here as well as profiteable. We have very much to do, but there is nothing here like strickness. I was lately matriculate, and with the help of my tutor, I escaped the oaths, but with much ado."

He thus began academic life at Oxford in good spirits but as a Non-juring Student. At this period Oxford University was not conspicuous for its intellectual activity. Fellows seem to have lived lives of comfortable ease without much regard to the requirements of the students under their care. As we shall see in Stirling's case, the rules imposed upon Scholars were very loosely applied, and, naturally, complaint was made at any stringency later. At the time we speak of political questions were prominent in the thoughts of both the students and College authorities.

The University had always been faithful to the house of Stuart. It had received benefits from James I., and for a time had been, during the Civil War, the headquarters of King Charles I., whose Cavaliers it remembered with regret when the town was occupied by the Parliamentary forces and had to endure the impositions of Cromwell.

At the time of Stirling's entry the reign of Queen Anne was drawing to a close. Partisan feeling between Whigs and Tories was strong, and of all the Colleges, Balliol was most conspicuously Tory. According to Davis (*Hist. of Balliol College*) Balliol "was for the first half of the eighteenth century a stronghold of the most reactionary Toryism" and "county families anxious to place their sons in a home of sound Tory principles naturally turned to Balliol," although from 1704 to 1721 Baron the master was a stout Whig.

It is therefore abundantly clear that Stirling had every reason to be content with his political surroundings at Balliol, with what results we shall see presently.

Perhaps the best picture of the state of affairs is to be gathered from the pages of the invaluable *Diary* of T. Hearne, the antiquarian sub-librarian of the Bodleian. For Hearne all Tories were "honest men," and nothing good was ever to be found in the "vile Whigges." His outspoken Tory sentiments led to his being deprived of his office and almost of the privilege of consulting books in the library, and yet he was on familiar terms with most of the scholars of his day. Luckily for us, Jas. Stirling was one of his acquaintance, and mention of Stirling's name occurs frequently enough to enable us to form some idea of his career. Doubtless their common bond of sympathy arose from their Tory, nay, their Jacobite, principles, but it speaks well for the intellectual vigour of the younger man that he associated with a man of Hearne's scholarship. Stirling must have been a diligent

student or he could never have acquired the scholarship that bore its fruits in 1717 in the production of his *Lineae Tertii Ordinis*, which is still a recognised commentary on Newton's *Curves of the Third Order*. But he was not the sort of man to let his light stay under a bushel, and he took a leading part among the Balliol students in the student riots that were to follow.

The accession of George of Hanover to the English throne was extremely unpopular in Oxford, and Hearne relates how, on May 28, 1715, an attempt to celebrate the King's birthday was a stormy failure, while rioting on a large scale broke out next day. "The people run up and down, crying *King James the Third*! *The True King*! *No usurper*! The Duke of Ormond! etc., and healths were everywhere drunk suitable to the occasion, and every one at the same time drank to a new restauration, which I heartily wish may speedily happen, etc. . . ."

June 5. King George being informed of the proceedings of the cavaliers at Oxford on Saturday and Sunday (May 28, 29) he is very angry, and by his order Townshend, one of the Secretaries of State, hath sent rattling letters to Dr. Charlett, pro-vice-chancellor, and the Mayor. Dr. Charlett showed me his this morning. This Lord Townshend says his majesty (for so they will stile this silly usurper) hath been fully assured that the riots both nights were begun by scholars, and that scholars promoted them, and that he (Dr. Charlett) was so far from discountenancing them, that he did not endeavour in the least to suppress them. He likewise observes, that his majesty was as well informed that the other magistrates were not less remiss on these occasions. The heads have had several meetings upon this affair, and they have drawn up a programme (for they are obliged to do something) to prevent the like hereafter; and this morning very early, old Sherwin the yeoman beadle was sent to London to represent the truth of the matter."

These measures had a marked effect upon the celebration on June 10 of "King James the IIId's" birthday. Special precautions were taken to prevent riotous outbreak. "So that all honest men were obliged to drink King James's health, and to shew other tokens of loyalty, very privately in their own houses or else in their own chambers, or else out of town. For my own part I walked out of town to Foxcomb, with honest Will Fullerton, and Mr. Sterling and Mr. Eccles, all three non-juring civilians of Balliol College, and with honest Mr. John Leake, formerly of Hart Hall, and Rich. Clements (son to old Harry Clements the bookseller), he being a cavalier. We were very merry at Foxcomb, and came home between nine and ten." Several of the party were challenged on their return, but no mention is made further of Stirling.

Rioting broke out again at Oxford on August 18, in which a prominent part was taken by scholars of Balliol.

There can be little doubt that Stirling was implicated in these disturbances, though he seems to have displayed a commendable caution on the 10th of June by going out of town with a man so well known as Hearne. His own account of events is given in the following letter to his father, which is the only trace of Jacobite correspondence with Scotland that has been preserved, if it can be so termed.

"Oxon, 23rd *July*, 1715.

Sir,

I wrote to you not long ago but I have had no letter this pretty while. The Bishop of Rochester and our Master have renewed an old quarrel; the Bishop vents his wrath on my countrymen and me in stopping the paying of our Exhibitions; it's true we ought to take Batchelours degrees by the foundation of these exhibitions, and quite them when we are of age to go into orders: Rochester stands on all those things, which his Praedecessours use not to mind, and is resolved to keep every nicety to the rigor of the statutes, and accordingly he hath stoped our Exhibitions for a whole year, and so ows us 20 lib. apiece. He insists on knowing our ages, degrees, and wants security for our going into orders. I suppose those things may come to nought in a little while, the Bishop is no enemy to our principles. In the meantime I've borrowed money of my friends, till I'm ashamed to borrow any more. I was resolved not to trouble you while I could otherwise subsist: but now I am forced to ask about 5 lib. or what in reason you think fit to supply my

present needs : for ye little debts I have I can delay them I hope till the good humor shall take the Bishop. I doubt not to have the money one time or another, it's out out of no ill will against us that he stops it, but he expects our wanting the money will make us solicite our Master to cringe to him which is all he wants.

No doubt you know what a generall change of the affections of the people of England the late proceedings hath occasion : the mobbs begun on the 28 of May to pull down meeting houses and Whiggs houses, and to this very day they continue doing the same, the mobb in Yorkshire and Lancashire amounted to severall thousands, and would have beat of the forces sent against them had they not been diswaded by the more prudent sort and they are now raging in Coventry and Baintry : so as the court saith the nation is just ripe for a rebellion ; etc.

Sir,

Your most dutifull Son,

J. STIRLING."

As Prof. Whittaker has informed me it was in the same year (1715) that we have the first indication of Stirling's power as a mathematician.

John Keill, of Oxford, in a letter to Newton of date Feb. 24, 1715, mentions that the problem of *orthogonal trajectories* which had been proposed by Leibniz had recently been solved by "Mr. Stirling an under-graduate here."

The statement commonly made that he was expelled from Oxford and driven to take refuge in Venice seems totally devoid of foundation. Again Hearne's *Diary* comes to our aid, and indicates that Stirling was certainly under the observation of the authorities of the government.

"1715, Dec. 30 (Fri.).

On Wednesday night last Mr. Sterling, a Scotchman of Balliol Coll. and Mr. Gery, Gentleman Commoner of the same College, were taken up by the Guard of the Souldiers now at Oxford, and not released till last night. They are both honest, non-juring Gentlemen of my acquaintance."

Also :

"1716, July 21 (Sat.).

One Mr. Sterling a non-juror of Bal. Coll. (and a Scotchman) having been prosecuted for cursing K. George (as they call the Duke of Brunswick) he was tryed this Assizes at Oxford, and the Jury brought him in not guilty."

The College records bear witness to the fact of his tenure of the Snell and Warner Exhibitions down to September 1716. As S.C.L. of one year's standing on Sept. 1715, and as S.C.L. in Sept. 1716.

There is no indication, however, of his expulsion, though the last mention we have of him in Hearne's *Diary* informs us that he lost his Scholarship for refusing to "take the Oaths."

"1717, March 28 (Fri.).

Mr. Stirling of Balliol College, one of those turned out of their Scholarships upon account of the Oaths, hath the offer of a Professorship of Mathematicks in Italy, w^ch^ he hath accepted of, and is about going thither. This Gentleman is printing a Book in the Mathematical way at the Theatre."*

VENICE.

Far from being expelled, he leaves Oxford with colours flying, though, as we shall see presently, after getting to Venice he found himself compelled on religious grounds to refuse the proffered chair.

The circumstances in which he had this offer are somewhat obscure : and whether he played any part in the Leibniz-Newton controversy is not certain. In the later stages of the controversy an intermediary between Newton and Leibniz was found in the Abbé Conti (1677-1749), a noble Venetian born at Padua, who, after spending nine years as a Priest in Venice, left the church and took up residence in Paris, where he became a favourite of Society. In 1715 he proceeded to London accompanied by Montmort. The two savants received a warm welcome from Newton and other English scientists. In

* The Sheldon Theatre, Oxford.

a letter to Brook Taylor, dated Paris 1721, Conti relates how "Mr. Newton me pria d'assembler à la Société les Ambassadeurs et les autres ministres étrangers." Conti and Nicholas Tron, the Venetian ambassador at the English court, became Fellows of the Royal Society at the same time in 1715.

How Conti came to meet Stirling is unknown to us, but he must have formed a high opinion of Stirling's ability and personal accomplishments, for Newton, in a letter quoted in Brewster's *Newton* (ii p. 308) querulously charges Conti with sending "Mr. Sterling to Italy, a person then unknown to me to be ready to defend me there, if I would have contributed to his maintenance."

The fact that Newton was a subscriber to Stirling's first venture, *The Enumeration of Cubics*, would suggest that Newton and Stirling met before Stirling had left England. This little book is dedicated to Tron, to whom Stirling acknowledges his indebtedness: and it was on the Ambassador's invitation that Stirling accompanied him to Venice with a view to a chair in one of the Universities of the Republic.

This book is sufficient evidence that he had not been idling at Oxford, and the long list of subscribers, nearly all of whom were either Fellows or Students of Oxford, bears eloquent testimony to the reputation which he had acquired in Oxford as a good mathematician.

It was printed at the Sheldon Theatre and bears the *Imprimatur*, dated April 11, 1717, of John Baron, D.D., the Vice-Chancellor of the University, and the (Whig) Master of his own College of Balliol, who was also subscriber for six copies. Forty-five of the subscribers are associated with Balliol College. Richard Rawlinson of St. John's is also a subscriber, and H. Clements, the Bookseller in London, takes six copies.

Thus Stirling leaves Oxford without a degree, but is the author of a work in Mathematics which earns him a reputation abroad. The invitation to Italy and the refusal are thus recorded in the *Rawlinson MSS.* in the Bodleian (materials collected by Dr. Richard Rawlinson for a continuation of Ward's *Athenae Oxonienses* up to 1750).

"Jacobus Stirling, e coll. Baliol, exhibit. Scot. a Snell jurament R.G. recus 1714, et in Italiam nobilem virum Nicolaum Tron Venetiarum Reipublicae ergo apud Anglos Legatum, secutus est ubi religionis causa Matheseos professorium munus sibi oblatum respuit."

The religious difficulty must have been a severe blow to Stirling's hopes and placed him in great embarrassment, for his means were of the scantiest. But adherence to the Anglican Church was one of the most important principles of the Tories, which had caused so much of their wavering for the Chevalier, who was a Catholic, and there was no getting over the difficulty for Stirling.

We need not be surprised, therefore, that he got into serious difficulties, from which he was rescued, in 1719, by the help of Newton, who thereafter, at least, had in Stirling one of his most devoted friends. Stirling's letter (1719) expressing his gratitude, is printed in Brewster's *Life of Newton*.

I have endeavoured to ascertain the university to which Stirling was called. Professor Loria has informed me that it was very probably Padua, Padua being the only University in the Republic of Venice,—the *Quartier Latin* of Venice according to Renan. It had been customary to appoint a foreigner to the chair of Mathematics. A foreigner * held it and resigned it in 1713. It was then vacant until 1716, when Nicholas Bernoulli was appointed. Professor Favaro of Padua confirms the above, and adds that possibly some information might be gathered from the reports of the Ambassador or from the records of the Reformatores Studii (the patrons of chairs in a Mediæval University). To get this information it would be necessary to visit Venice.

My chief difficulty here is to reconcile the dates of Stirling's visit to Italy and the date of the vacancy. It may be added that a College for Scotch and English students still flourished at Padua about this time. But in 1719 Stirling was certainly resident in Venice, and he is known in the Family records as "The Venetian."

Life in the cultured society of Venice must otherwise have been very congenial. It was a favourite haunt of the different members of the Bernoulli

* Hermann.

family and of Goldbach; and the earliest letter of a mathematical nature to Stirling is one in 1719 from Nich. Bernoulli, F.R.S., then Professor of Mathematics in the University of Padua. One is tempted to enquire whether Stirling did not meet Bernoulli and Goldbach when they visited Oxford together in 1712. The letter in question by N. Bernoulli specially refers to their meeting in Venice, and also conveys the good wishes of Professor Poleni, Professor of Astronomy in Padua. At this time also Riccati was resident in Venice, which he refused to leave when offered a chair outside Venice.

How long Stirling lived there after his letter to Newton was written is not known, nor is the cause of his difficulty which he does not trouble Newton with having already given an account to Desaguliers. While in Venice he sent to the Royal Society his paper on the *Methodus Differentialis*, printed in the *Philosophical Transactions* in 1719.

London.

From 1719 to 1724 there is a gap in our information regarding Stirling. But a fragment of a letter of his to his brother, Mr. John Stirling of Garden, shows that he was at Cader in July 1724. Certainly early in 1725 he was in London, as a letter, somewhat imperfect, to Mr. John Garden informs us (London, 5th June, 1725). He was then making an effort towards "getting into business. It's not so easily done, all these things require patience and diligence at the beginning. In the meantime, that he may not be 'quite idle,' he is preparing for the press an edition of . . . * Astronomy to which he is adding some things; but for half a year the money will not come in, and he hopes his mother will provide towards his subsistence.

"So I cannot go to the country this summer but I have changed my lodgings and am now in a French house and frequent French coffee houses in order to obtain the language which is altogether necessary. So I have given over thoughts of making a living by teaching Mathematiks, but at present I am looking out sharp for any chub I can get to support me till I can do another way. S. Isaac Newton lives a little way of in the country. I go frequently to see him, and find him extremely kind and serviceable in every thing I desire, but he is much failed and not able to do as he has done. My first way of living was expensive etc. . . . Direct your letters to be left at Forrest's Coffee House near Charing Cross."

Thus in 1725, at 32 years of age, Stirling had not yet found a settled occupation which would furnish a competency. Some time after he seems to have taken a share in Watt's Academy in Little Tower Street (in which Thomson, the poet, wrote part of "The Seasons" while attached as a tutor in 1726). It was to this address for the next ten years that the letters to Stirling (that have been preserved) from contemporary mathematicians were directed.

They form part of a larger collection that was lost by fire. Those that were saved nearly went astray altogether in the nineteenth century, through the carelessness of Sir John Leslie of Edinburgh University, for whom they had been obtained by Prefessor Wallace. They were returned only after the death of Leslie by his executors.

There are also a few letters to his friends in Scotland, and from these we can gather a certain amount of information. In the earlier days of his struggle in London he had to depend upon his relatives for assistance, but as his affairs improved he showed as great, or greater, generosity in return. By 1729, he could look forward with confidence to the future, for by that time he was able to wipe off his indebtedness in connection with his installation in the Academy, as the following extracts show.

In April 1728 in a letter to his mother he writes: "I had 100 lib to pay down here when I came first to this Academy and now have 70 Lib more, all this for instruments, and besides the expences I was at in fiting up apartments for my former project still ly over my head."

Again on 22nd July, 1729, he writes: "Besides with what money I am to pay next Michaelmas I shall have paid about 250 Lib. since I came to this

* The name is torn out.

house, for my share of the Instruments after which time I shall be in a way of saving, for I find my business brings in about 200 L. a year, and is rather increasing, and 60 or 70 L. serves me for cloaths and pocket money. I designed to have spent some time this summer among you, but on second thoughts I choose to publish some papers during my Leisure time which have long lain by me. But I intend to execute my design in seeing you next summer, if I find that my affairs will permit."

He had always a warm side for his friends in Scotland, and his letters to them are written in a bright and cheerful style. The reference to Newton is the only one he makes regarding his friends at the Royal Society, and the "papers" he speaks of publishing are almost certainly his well known Treatise the *Methodus Differentialis* (1730), the first part of which he had drawn up some 8 or 9 years before (*v.* Letter to Cramer).

He was admitted to the Royal Society in 1726, a distinction that put him on an equal footing with the Scientists that lived in, or frequented, London. It is most probable that his acquaintance with Maclaurin began at this time. They were both intimate friends of Newton and fervent admirers of his genius, and both eagerly followed in his foot-steps. The correspondence between them begins in 1728 when Maclaurin and Stirling had taken an interest in G. Campbell, with whom Maclaurin was to have such an unfortunate dispute.

Maclaurin placed great reliance upon Stirling's judgment, and frequently consulted him while his *Treatise of Fluxions* was in the press. The later letters that passed between Stirling and Maclaurin have much to do with their researches upon the Figure of the Earth and upon the Theory of Attraction.

In 1738, Stirling, at Maclaurin's special request, joined the Edinburgh Philosophical Society, which Maclaurin had been so instrumental in founding in 1737. Maclaurin also begged for a contribution, but if Stirling gave a paper to the Society it has not been preserved and printed.

In 1727, Gabriel Cramer, Professor of Mathematics at Geneva, received a kindly welcome from the Royal Society on his visit to London. He formed a warm friendship for Stirling, who was his senior by about twelve years, and several of his letters to Stirling are preserved. A copy of a letter to him from Stirling has been preserved and furnishes interesting information regarding Stirling's own views of the *Methodus Differentialis* which appeared in 1730, and also regarding the date at which the Supplement to De Moivre's *Miscellanea Analytica* was printed. Stirling had sent two copies of his work to Cramer, one of the copies being for Nich. Bernoulli, by this time Professor of Law at Bâle. Letters from Bernoulli to Stirling were sent through the hands of Cramer, who thus obtained the benefit of the correspondence as well. There are a few letters from N. Bernoulli, the last bearing the date 1733. In this letter, while pointing out Errata in Stirling's book of 1717, Bernoulli observes the omission made by both Stirling and Newton of a species of a Cubic. In Newton's enumeration 72 species are given. Stirling in his little work of 1717 added four more. But there were still two more species, one of which was noted by Nicol in 1731. The discovery of the remaining one announced by Bernoulli in 1733 gives Bernoulli precedence over Stone's discovery of it in 1736. Murdoch in his *Newtoni Genesis Curvarum per Umbras* mentions that Cramer had told him of Bernoulli's discovery, but does not give a date. The letter to Stirling not only confirms Cramer's statement, but its date gives Bernoulli undoubted precedence over Stone.

About this time Stirling's life must have been one of considerable comfort, as his business was prosperous, while he was an active member of the Royal Society where his opinions carried weight.

He was not a prolific writer, and what he did write was usually devoid of any needless verbiage. After the publication of his book in 1730 he turned his attention more particularly to the study of the Figure of the Earth, regarding which there was considerable discussion, there being two theories, (1) that of Newton that the Earth was flatter at the Pole than at the Equator, and (2) the theory of the Cassinis, who held exactly the opposite view. Stirling wrote for the *Philosophical Transactions* a short but important note on the subject which led Clairaut to seek correspondence with Stirling after the publication of a paper in the *Philosophical Transactions* on the same subject in which he had come to similar conclusions.

Euler's correspondence with Stirling is of a later date (1736-38), and has already been referred to by me in the *Mathematical Gazette*.

The last of the letters of a scientific character is one in 1747, in which Folkes, P.R.S., informs Stirling that he (Stirling) has been made a member of the Prussian Academy. It contains an interesting reference to the aged De Moivre.

Return to Scotland.

In 1735, a great change occurred in his fortunes by his appointment to the Managership of the Lead Hills Mines in Scotland, and from 1735 onwards we may follow his career with more certainty, there being fairly full records of the part he played in Scotland, in which, in the opinion of some, he eclipsed his earlier fame as a scholar by the skill of the administrative ability he displayed in his control of the mines.

From this point on we have much more definite information regarding Stirling's life. In particular, he is taken as one of the best types of the Scotchmen of his day by Ramsay in his *Scotland and Scotsmen*. Ramsay met him frequently when Stirling was on visits to his relatives at Keir and elsewhere, and had a profound regard for the courtly and genial society of Stirling. Stirling had had a wide experience of the world from the academic atmosphere of Oxford with its political strife, and the scholarly life in Venice, where he made numerous friends among the Italian *savants*, to his life in London and the scientific associations of the Royal Society. He had thus acquired a deep insight into human character, and he was a *persona grata* in the best society of Scotland. Ramsay narrates a number of anecdotes illustrating the kindly humour of Stirling.

Before Stirling left Venice he had, at the request of certain London merchants obtained information regarding the manufacture of plate glass. Indeed, it is asserted by some that he had to flee from Venice for fear of assassination for his discovery, though Ramsay makes no mention of it. In London, on his return, this paved the way for an acquaintance with a number of merchants, who formed a high opinion of his abilities and led to his selection as Manager of the Lead Hills Mines for the Scots Mining Coy. about 1735. This Company had been founded some twenty years before by Sir John Erskine of Alva. Most of the capital came from London merchants associated with the Sun Fire Office. The Company had for its object the development of the metal mining in Scotland. They had taken various leases in different parts of the country, but these were all given up with the exception of the lease of the Lead Hills Mines, the property of the Hopetoun family, which had been worked for more than a century. The manager for them was Sir John himself, but though he was a man of genius, he failed in the business capacity to put his ideas into practice on a remunerative footing, and, when Stirling was appointed, the affairs of the Company were at a low ebb. The appointment, though a strange one, was fully justified by its success.

For the first year or two Stirling only resided at the mines for a few weeks to arrange matters with the miners, but about 1736 he took up definite residence, devoting his energies entirely to the interests of the Company.

Bit by bit the debts which had accumulated in his predecessor's time were cleared off, and the mines became a source of profit to the shareholders.

We find him in his letters to Maclaurin, with whom he still frequently corresponded, complaining that he had no time to devote to their researches, and often writes in haste; and, when writing to Euler, he tells Euler that he is so much engrossed in business that he finds difficulty in concentrating his thoughts on Mathematical Subjects in the little time at his disposal.

The village in which he and the miners resided was a bleak spot in bare moorland, nearly 1300 feet above sea level. There was no road to it, hardly even a track, and provisions and garden produce had to be sent from Edinburgh and Leith. In spite of these disadvantages Stirling has left indelible traces of his wise government of the mines and the miners, and many of the improvements have a wonderful smack of modernity.

The miners at the time of his appointment were a rough disreputable set of men, who had good wages but few of the comforts of life. Stirling's first care was to add to these comforts and to lead them by wise regulations to attend to their own physical and mental requirements.

In the first place he grouped the men in four classes: (1) Miners proper, each older miner being allowed an apprentice at his own cost and profit; (2) Labourers; (3) Washers, with boys under them; (4) Smelters.

The men worked in shifts of six hours, so that with a six hours' day they had ample time at their disposal if they wished. By a system of payment by results the men were willing to do their best to promote production.

In order to turn their leisure to profit Stirling took advantage of a clause in the lease which allowed the Company to provide the miners with suitable gardens. Thus a miner could trench and plant practically as much land as he found it possible for him to work, there being no statement as to the size of the "allotment." Under careful cultivation the gardens or crofts produced fair crops, and assumed a value in which the miner himself had a special claim, so much so that he could sell his right to the ground to another miner without any interference being feared from the proprietor. Stirling thus stimulated the industry and economy of the miners, while at the same time furnishing them with a restful and healthy relaxation from their underground toil.

The miners were also subject to a system of rules which were drawn up for their guidance, and by which disputes could be amicably settled. They had to make contributions towards provision for their sick and aged,—in other words, they formed an Insurance Society.

For their mental improvement he instituted, doubtless with the help of Allan Ramsay, who was a native of the place, a library, to the upkeep of which each miner had to make a small subscription. He is thus an early precursor of Carnegie in the foundation of this—one of the first free libraries. When Ramsay of Ochtertyre visited it in 1790, it contained several hundred volumes in the various departments of literature, and it still exists as a specimen of Stirling's provision for the men under his care.

Stirling's own requirements, on the other hand, were well provided for by the Company, whose affairs were so prosperous under his management. They saw to it that he was well housed. They more than once filled his cellar with wine, while the salary he obtained enabled him to amass what he considered a considerable competency. When he began to fail with the increase of years they supplied him with a carriage.

The Rebellion of 1745.

Stirling does not appear to have taken any part in the rebellion of 1745. His Uncle and Cousin of Keir were kept out of it by their apprehension and imprisonment. It may be questioned whether he was still a Jacobite: for in London he was one of the brilliant coterie that surrounded Bolingbroke on the return of the latter from exile, and if he at all shared the politician's opinions he would realise that a Stuart restoration was hopeless. (Stirling most esteemed Berkeley of all the men in this set.)

If I recollect aright, Carlyle in his *Autobiography* relates that, on the death of Maclaurin in 1746, he was spoken of as a likely successor, only the feeling against anyone connected with the Jacobites was too strong to admit of his nomination.

In 1754 he demitted his fellowship of the Royal Society.

The Glasgow Kettle.

A new sphere for Stirling's activities arose in connection with the development of the city of Glasgow.

Towards the middle of the eighteenth century the trade of the city with the West Indies had undergone enormous expansion, and great sums were spent in improving the River and Harbour. A separate account was kept of these expenses, and the first item of the Ledger is dated 1st July, 1752, and runs thus:

"Paid for a compliment made by the Town to James Stirling, Mathematician, for his service, pains, and trouble, in surveying the River towards deepening by locks, viz.:

For a silver Tea Kettle and Lamp weighing 66¼ oz.

At 8s. per oz. - - - - - - -	£26	10	0
For Chasing and Engraving the Town's arms	1	14	4
	£28	4	4

Apparently Stirling had given his services gratuitously. The Kettle is still at Garden, and on festive occasions the City rejoices to exhibit it in memory of its gratitude to Stirling for his services.

By his marriage with Barbara Watson, daughter of Watson of Thirty Acres near Stirling, he had one daughter Christian, who married her cousin, Archibald of Garden, Stirling's successor as manager of the Lead Hills Mines. The estate of Garden is in the possession of their descendants.

Stirling died in Edinburgh in 1770, and he lies buried in Greyfriars Churchyard, where his friend Maclaurin had long before him been laid to rest. (It also contains the tomb of Matthew Stewart, Maclaurin's successor.) The spot where he is buried cannot be difficult to locate, for the Register Records describe it somewhat grimly as "Twa corpse-lengths west of Laing's Tomb," Laing's tomb being a large mural monument (1614) on the wall to the right on entering the Churchyard.

Thus closed a career filled with early romantic adventure and brilliant academic distinction, followed in later years by as marked success in the industrial field. As a Mathematician, Stirling is still a living power, and, in recent years, there has sprung up, more particularly in Scandinavian countries, quite a Stirling cult. His is a record of successful achievement of which any family might well be proud.

Note.—In obtaining information regarding the life of Stirling I have had the kind assistance of many friends and correspondents. I am particularly indebted to Mr. Henry Barker of Edinburgh University; Professor Gibson, Glasgow; Mr. Pickard-Cambridge of Balliol College; and Mr. F. Madon of the Bodleian Library, Oxford. C. T.

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